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THE PSYCHOLOGICAL BULLETIN

SLEEP

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The studies which are covered in this review may be grouped as follows: (1) those which employ the method of sensory stimulation; (2) those which employ registration of changes in bodily position; (3) those which employ measurement of the rate of oxygen-consumption; (4) those which depend primarily on histological examination of tissues; (5) some theoretical contributions; and (6) a few miscellanies.

1. *Studies based on the method of sensory stimulation*

This method is probably the first one that was used in a controlled experiment. Fundamentally it consists in measuring the magnitude of the stimulus which is found necessary for awakening the sleeper, the criterion of *awakening* being arbitrary, but in theory, constant. The results based on this method form the basis of most of the discussion of sleep to be found in the medical, physiological, and psychological textbooks.

As a preliminary to the publication of three years' experimental work, based on an entirely different method, the present reviewers have made complete written translations of the original reports of Kohlschütter (20), Mönninghof and Piesbergen (30), Michelson (29), and Czerny (5); and have availed themselves of the translation of the study of de Sanctis and Neyroz (34) made by Warren. They have checked the original exhibits of the data, compared them with the texts of the reports, and in some cases have reduced the data to other forms of description for the better com-

parison of the results. This procedure disclosed that the current works on sleep, including special treatises as well as textbooks, abound in misstatements of fact concerning the studies just named, as well as in uncritical and sometimes preposterous interpretation. The explanation may lie in the fact (which is rendered clear from internal evidence) that most of the comparatively recent writers have not read the series of reports which they cite, but have depended on the accounts of other authors, who likewise have not read them. We shall not enter into detail here concerning the history of some of these errors, although some of them are easily traced, and the recital would make an interesting story. However, the fact that some of the original reports are now hard to get, together with the fact that they deal with important questions, makes it seem profitable to publish in 1930 a review of work begun in 1859, although the reports can hardly be classed as current literature.

Certain of these studies (20, 29, 30) were the dissertational experiments of candidates for the degree of doctor of medicine; others were made by young doctors during their service as internes and assistants. Possibly the later engrossment of the authors in the details of medical practice may account for their failure to follow up some of the exceedingly interesting questions which they propose, during the discussion of their experimental results, as problems for future attack.

The background of this group of studies was set by Kohlschütter (21), who was inspired by his teacher Fechner (11). It is interesting, whether pertinent or not, to note that the results which Kohlschütter presents in the form of an ideal curve of the "depth of sleep" (so frequently reproduced in textbooks and dissertations) fit very accurately the *a priori* description given by Fechner (11, II, 433 ff) of the diurnal course of the fluctuations of "general consciousness." We shall presently see that this concordance was accomplished by Kohlschütter's disregard, on very questionable grounds, of numerous measurements which deviated widely from the expectation which Fechner had raised.

All these studies rest upon three considerations, some of which are not expressly stated, though definitely implied. We shall attempt to state them; not in the exact form which the original authors would have used, but in one which, at some points, is more simple than Fechnerian terminology permits.

- (1) The notions of *sleep* and of *waking* are strictly *relative*;

first, to the particular *stimuli* which are being considered; and next, to some particular *kind of response*, which is *arbitrarily* taken as an indicator of the efficacy of those stimuli. Corresponding to each presentation, the experimenter enters a categorical judgment: *i.e.*, that the critical response occurred, or that it did not.

Other conditions being constant, the less work a stimulus has to perform on the patient's sense-organs to effect the critical response, the more *awake* the patient may be said to be, *with respect to that particular stimulus*. If the minimum effective input, under standard conditions is b , and under compared conditions is b' , the relative degree of waking (*with respect to the particular stimuli employed*) may be defined as some function of b and b' , and its negative treated as the "depth of sleep," with respect to the same stimuli. It is important that the qualifying phrases be preserved, even at the risk of tedium, for their disregard has occasioned grave confusion in thought and discourse for some 68 years.

We may further observe that such a treatment yields only a fact of definition: it provides us with an additional way of talking about the facts of observation, a way which resembles the every-day manner of speaking, but that is all.

Recalling that "sensitivity," "*Empfindlichkeit*," "irritability," "responsivity," *etc.*, are names given to some function or functions of the ratio between the *magnitude*¹ and the *effectiveness*² of the stimulus concerned, we may relate the two facts of definition and identify the degree of waking with some function of the sensitivity of the subject or the "effectivity" of a standard stimulus; and call the negative of this formal entity the "depth of sleep," with respect to the stimulus in question.

These definitions, though unnecessary, are respectable, and for some purposes may be convenient. But, they denote nothing, not even the existence, of *introspectively observable sensations*, until such a reference is attached by an additional *assumption*. That assumption is the subject of our next consideration.

(2) Fechner defined³ the intensity γ of the consciousness aroused by a particular stimulus, as follows:

$$\gamma = k \log (\beta/b)$$

¹ Measured in *physical units*.

² Determined by *classifying responses* according to some conventional standard, *counting* the responses in each class, and expressing the numbers relatively.

³ Fechner often referred to this relationship as being empirically derived.

in which k is a constant of proportionality, β the magnitude of the stimulus, and b that value of β which has standard effectiveness in the sense given in our preceding footnote. If $\beta < b$, then $(\beta/b) < 1$; its logarithm is negative, so that the intensity γ of the corresponding item of consciousness is also less than "zero." This "zero," however, is not an absolute *null*, indicating a complete absence of *sensation*; it is merely an arbitrary reference-value. As Kohlschütter observes (20, 210ff.), the use of plus and minus signs means simply that the magnitude of γ exceeds or falls short of an arbitrarily chosen, finite value, by the amount indicated by the numerical part of the expression. A negative value of γ indicates *sleep*, in Fechnerianism, with respect to the stimulus in question. In plain words, it means that the stimulus is too small or too feeble, at the time, to arouse the conventional response as frequently as an arbitrary standard requires. Such a condition could occur from diminishing the value of β or from diminishing the patient's sensitivity and thus increasing the value of b . This interpretation summarizes all the empirical facts which are covered by Fechner's description of sleep as a condition of "*negative consciousness*"; although that description, being an obscure utterance of a great man, has often been treated as profound. All the empirical facts presented by Kohlschütter could have been stated fully and yet simply, without reference to the assumption implied in the *Massformel*. The fact is important, for the application of the latter formula to "*general*" consciousness as well as to the kind of *sensation* which it defines, rests upon still another assumption, which more recent experiment has shown to be invalid: namely, that the energy released as a result of the action of the stimulus upon the organism is proportional to the magnitude of the stimulus! Whatever validity the results of Kohlschütter *et al.* may have, can be more readily determined if the results are considered independently of the invalid assumption that determined the choice of descriptive terms.

(3) If the "depth of sleep," defined according to consideration (1), and taken with respect to a particular complex of stimuli, be treated as if it represented the depth of "*general sleep*," the treatment implies an assumption: namely, that in sleep, the "*effectivities*"

It is, in fact, based on an *interpretative* statement of Weber's law, which proves from analysis to be a defining equation and not an empirical relationship. This analysis is presented in detail in an article entitled "Did Fechner Measure 'Introspectional' Sensations," by one of the present writers (J), published in the *Psychological Review* in 1929.

of all stimuli diminish in the same proportion, or that one's sensitivity to all stimuli has diminished in equal degree. This assumption is explicitly denied by Fechner and also by Kohlschütter. It is restored by implication, however, by Kohlschütter, Mönninghof and Piesbergen, Michelson, and Czerny, for while none of them pretended to measure anything but the varying depth of sleep with respect to particular noises or electrical currents, all of them discuss the results as though they exhibit the variations of "general" sleep.

To express the "depth," at any moment, of what these definitions would require us to call "*general sleep*," it would be necessary to measure, separately and simultaneously, the effectivity of each member of the universe of stimuli that are playing on the organism at the moment, and to weight and combine the numerical expressions of the measurements in a form which could be stated. The procedure would have to be repeated for each temporal instant included in the comparison. Since fulfillment of these conditions is physically impossible, we may be assured that for practical purposes, there is no such thing as "*general sleep*," in a sense which satisfies Fechner's definitions.

The more recent writers refer to the experimental studies which we have listed above as if they were made on one and the same variable. This treatment appears to arise from the fact, that each of the authors, having obtained a series of numbers which expressed the results of measurements of *stimuli*, named the numbers expressions of the "*depth of sleep*."⁴

As a matter of fact, each of these studies was confined to a single sense-field, with a single type of stimulus appropriate to that field. Moreover, in the three studies (20, 30, 29) which employed the special sense of hearing, the values presented by the different authors do not even represent the products of the same dimensions. This point is important to proper interpretation of the results; we shall therefore enter into a few details for the sake of developing it.

⁴ The fallacy of treating two variables as identical because they happen to be called by the same name is known in Logic as *Equivocation*. It occurs very frequently in the literature of all branches of endeavor which do not rigorously follow the rules of scientific method. It curses the descriptions and the teachings of medicine, physiology and psychology; it forms the basis of many of the so-called 'problems' of philosophy; it generates most of the absurdities of the mental testing movement, as well as the commercial success of the latter. Its use is a grave immorality, which requires careful watching for prevention and detection.

Kohlschütter (20) employed a sound-pendulum constructed by Fechner. It is not, however, either of the two which Fechner describes in his *Elemente* (11). Kohlschütter's pendulum having been raised to a height indicated on a graduated arc and released, its bob, or hammer, struck against a block of slate at the bottom. The weight and material of the hammer and the dimensions of the block of slate are not given: hence, one could not hope to reproduce, even crudely, the characteristics of the sound-wave which the apparatus generated. The location of the apparatus was not equally distant from the two ears of the observer. Kohlschütter found that the auditory acuity was not the same for the two ears. He therefore segregated the results according as the right or the left ear was nearer to the instrument, and took account, in a way to be mentioned presently, of the distance of the instrument from the nearer-lying ear. He disregarded, however, the angle at which the external opening in the ear was inclined to the direction of the sound-wave; and also disregarded whether the one or the other ear might be occluded, as by the pillow.

The angle of elevation ψ being read directly, the height of fall, h , may be derived from the identity given in elementary Geometry:⁵

$$h \equiv 1 - \cos \phi \equiv 2 \sin^2 \frac{1}{2} \phi$$

The total energy of impact is given by Mh , in which M represents the mass of the hammer, and h the height of fall. Kohlschütter assumed that a constant fraction of the energy of impact is converted into sound. Vierordt, a physiologist, disputed this assumption, and in a private communication to his pupils Mönninghof and Piesbergen, presented experimental evidence, the character of which they did not give, to support his criticism. Our colleagues in physics inform us that the settlement of the question would be extremely difficult by experimental methods now available. It would have been still more difficult of settlement by the methods which were available in Vierordt's day. Kohlschütter assumed further that the energy delivered by the sound-wave to the ear varies inversely as the square of the distance between the source and the ear, even in the closed room in which he worked. Obviously, the energy in the primary wave would be augmented by reflection from the walls, etc. As he could not ascertain how great this augmentation was, he assumed it

⁵ This equation is misprinted in Fechner's discussion, and is so badly set in Kohlschütter's article as to invite misreading by one who did not pause to reason the matter out (11, 180; 20, 214 f.).

to be zero, thus exemplifying another fashion in procedure which experimentation sometimes justifies and sometimes overthrows. (If the augmentation factor were constant, it would not affect the relative values of the results. It would be constant if the positions of the ear and of the generator were constant with respect to the walls, floor and ceiling of the room, as well as with respect to each other, and if the energy-frequency distribution in the sound-waves were independent of the height of fall, or else the absorption of the walls, etc., were non-selective.)

On the basis of facts and assumptions combined, Kohlschütter reasons that his stimulus-intensities, measured by the work performed on the ear-drums of the patient, were proportional to

$$I = (288 \sin^2 \frac{1}{2} \phi) / e^2$$

in which e represents the distance of the block of slate from the nearer ear, measured in the Leipzig foot which contains 31.3 cm. The factor 288 should be proportional to the weight of the pendulum-hammer.

Mönninghof and Piesbergen (30) employed as a sound-generator a leaden ball, flattened in several places to prevent rolling. It weighed 16.2 grams, and fell freely from a variable height upon an iron plate $20.4 \times 16.7 \times 0.55$ cm. They assumed that the energy converted into sound was not proportional to the product Mh of the mass and the height of fall, but proportional to $Mh^{0.59}$. For the value of the exponent they relied on the communication of Vierordt, mentioned above. They also assumed that the energy delivered to the receiving surface varied, not as the inverse of the square of the distance from the source, but inversely as the first power of the distance. As the mean distance they employed was 150 cm., they considered such deviations to be negligible as might be introduced by the patient's changes of bodily position, and therefore took no account of them. They also disregarded whether one ear or the other was nearer the source of sound, and also whether one or both ears may have been covered, and what might be the angle of inclination of the effective ear with respect to the direction of the sound-wave. Four threshold-determinations which they made on each other yielded a mean value of 34.13 for the product $Mh^{0.59}$, in which M was measured in milligrams and h in millimeters. Desiring to express their results relative to this mean threshold, they called their variable products $Mh^{0.59}/34.13$ measures of sound-intensities.

Michelson (29) used 14 brass balls, whose weights ranged between 5 and 100 grams. Despite the presence of variables which might effect a change in the order of magnitude of a threshold determination, he expresses the observed weights in milligrams, or to four places of decimals in one extreme case and to six places in the other. He selected and released these balls by means of an ingenious electro-mechanical device which operated without noise and which was controlled from a distant room. The balls fell upon a sounding-board of oak, $25 \times 25 \times 3.5$ cm., inclined to the line of fall, so that after impact, the ball was reflected into a padded box. The mean distance from the patient was 100 cm., which was treated as constant. Disregarding all other factors, Michelson assumed the sound-intensities to be proportional to the product Mh , expressed in grams and centimeters respectively.

From the foregoing description it can be seen that there is now no reliable means of reducing these three sets of results to a comparable basis.

Czerny (5) worked on children, using electrical stimuli produced by a DuBois-Reymond inductorium. The secondary coil was fixed with reference to the primary. The current through the latter was supplied by a battery of LeClanché cells, varied by means of an adjustable resistance, and measured on a milliammeter. The child was made part of the secondary circuit by means of flexible connectors which terminated in metallic zinc plates, secured to the upper arms of the child by inelastic bands. An extra switch permitted the secondary circuit to be shorted around the child while the preparations were being made. The latter being closed, the primary circuit was closed, the rheostat adjusted to give a predetermined reading, and the short-circuiting switch opened. The primary circuit was then broken by a manual switch. The procedure was repeated with progressive increases in the primary current-strength until the child made a response which the experimenter regarded as critical.

It has been mentioned that the primary current was measured during the period of steady flow. Czerny *assumed* that the strength of the secondary current induced by the break was proportional to the strength of the steady primary current. He therefore presented the numerical expressions of the latter as relative measurements of his *stimulus-intensities*. As far as we have yet seen, this interpretation has not been questioned by the numerous writers who refer to

his experimental report. If a little regard be paid to the physical conditions, the interpretation will prove to be invalid.

Under the conditions described, the intensity of the actual *stimulus* applied would be proportional to the *density*⁶ of the current which passes through the receptors in the very brief time during which the inductive discharge takes place.

This would be affected to a degree which is worthy of a first order of consideration, by three conditions which were actually variable, but which Czerny treated as constant.

(1) The first condition is the *effective area of contact*. The form of the upper arm does not remain constant; that of the electrodes does. The area of contact depends on the area of skin which conforms to the metal. The conformation may be altered in two ways: (a) by changes in the form of the arm which accompany changes in posture; and (b) by changes in the volume of the arm, for if the arm swells, a larger area of skin is brought into intimate contact with the metal, whereas this area is diminished when the arm shrinks. (It will be recalled that the bands which fastened the plates to the arm were inelastic.) It is easy to see that the form of muscular tissue does not remain the same for all postures, and Howell (14) has shown that the volume undergoes marked fluctuations during the time devoted to sleep. A constant *current* if sent through a large area of skin, containing many receptors, would yield a smaller *current-density* than if it were sent through a smaller area, containing fewer receptors. If the *current* is comparatively weak, the result, in the first case, might be subliminal stimulation of many receptors, resulting in failure of the critical response; and in the second case, the effective stimulation of fewer receptors, resulting in production of the critical response.

(2) The second condition is the resistance between the skin and the electrodes. This factor would be affected by two agents which are known to vary, not to speak of others, such as film-surfaces, of which we know but little. (a) The intimacy of contact between skin and electrode partially determines the resistance, so does (b) the moisture of the skin and the concentration of salts in the film of sweat between skin and metal. Of the variations in the former during the course of the experiment, we know nothing except that they were not prevented and should not be ignored. Of the latter,

⁶ Measured in amperes per square centimeter.

as we shall presently see, Czerny has told us something, of which he took no adequate account.

(3) The third condition is the leakage by conduction over the *surface* of the skin. This leakage would be small if the skin-surface which separated the two electrodes were dry: but it would certainly increase if the skin were rendered moist by perspiration. Czerny actually measured the rate of perspiration of the skin of the forehead in two experiments, and presents the results. The latter may not reliably indicate the rate of perspiration in the skin of the arms and trunk, or enable us to estimate the humidity of the latter, but they do show us that in the skin of the forehead, the rate is by no means constant. In one experiment [17], the range of variation was of the order of 76 to 1; in the other experiment [18], of the order of 62 to 1. *The heaviest currents through the primary coil were required for awakening at those times of the night when the rate of perspiration was the highest, and conversely.* Czerny's interpretation of this finding is that the stimulus-intensities remain constant, but that the patient became less sensitive when the skin was moist: our suggestion is that under such conditions the intensities themselves were reduced by leakage more than they were increased by diminished resistance through the skin. We do not argue that the leakage was sufficient to account for all the differences which Czerny got; in fact, we suspect the contrary from considerations independent of his experiment; but the point can be settled only by direct measurement. But we do contend that to assume that the stimulus-intensities were simply proportional to the primary current-strengths is to neglect some variable *factors* which cannot be safely disregarded; their consideration might greatly change the form of the curves employed to express the results.

In this connection we may remark also that the key in the primary circuit was opened by hand; and that the characteristics of the current induced by the break are by no means independent of the speed with which the two contacts are separated.

De Sanctis and Neyroz (34) employed pressure-stimuli, obtained from the blunt point of an æsthesiometer, applied to the patient's forehead. The total time allowed for each stimulation was 20 seconds; the experimenter noted the position of the pointer at the time of occurrence of the first non-specific response, such as a facial contortion, and of a response considered as appropriate. Thus, in addition to the uncertainty which attends the reading of a moving

pointer while one is also watching something else, the readings were subject to the variability of the patient's and the experimenter's reaction-time. These authors plotted these readings under the label "depth of sleep."

In respect to *experimental procedure*, this group of studies presents an equally interesting divergency.

Kohlschütter awoke his patients as often as seventeen times during a single night. Beginning with an ineffective angle of elevation of the pendulum-hammer, he caused the latter to strike six times upon the block, at intervals of one second. If the critical response did not occur, he increased the angle by 5° and repeated the procedure, thus continuing until he found an effective elevation. He encountered the obvious criticism that such frequent application of the stimulus must affect the natural course of sleep. He admitted that this defect is inherent in the procedure; but he discounted it in some degree by arguing that most of the stimuli were subliminal, and that it is impossible that a subliminal stimulus, however oft repeated, can summate to effectiveness.⁷ His argument is now well known to be inconsistent with fact. Despite his insistence, however, he actually took some account of effects of previous stimulation in his manner of *weighting* the maximum ineffective and the minimum effective stimulus-values in computing the threshold. Among the effects which influenced the weighting were changes in the character of respiration. Reed and Kleitman (33) report that such changes are unrelated to changes in specific sensitivity.⁸

Kohlschütter's work has been criticized on the further ground that the presence of the experimenter in the sleeping room, and the

⁷ "Eine Summierung der in der Zeit getrennten, für sich unzureichenden Eindrücke zu einem stärkeren findet hierbei nicht Statt. Denn dass ein noch so häufiges und noch so rasches Wiederholen einer an sich zu geringen Intensität nicht Erwachen bewirken kann, davon habe ich mich durch spezielle Versuche überzeugt. Man kann eine solche fünfzig- bis hundertmal in rascherem oder langsamerem Rhythmus einwirken lassen, ohne dass Erwachen erfolgt. Selbst ganz nahe an der Grenze vermag die öftere Wiederholung nicht die absolute Insuffizienz auszugleichen" (20, 220).

⁸ Michelson misrepresents Kohlschütter's procedure in this respect, declaring that the former accepted very slight changes in respiration as signs that the patient had awakened. Kohlschütter (20, 218) tells us: "Als eigentliches Kriterium des Erwachens habe ich aber nur die Laute welche vorhergangener Verabredung gemäss der Geweckte ausstieß, betrachtet." Michelson's error has been propagated by later authors, who appear to have relied on his account instead of consulting the original.

illumination in the latter, may have introduced still more disturbance. The same objectionable features attended the work of Mönninghof and Piesbergen (30), de Sanctis and Neyroz (34), and apparently also of Czerny (5).

Mönninghof and Piesbergen (30) presented three sets of stimulus-values for each single determination: namely, (a) the minimum found to be effective; (b) the sum of those found to be ineffective; and (c) the ratio a/b . It is thus evident that they did not measure thresholds, although, since they proceeded by equal increments of height, they left the calculation of threshold-values possible. They presented each stimulus but twice, and made not more than two experiments on any one night. By so doing they obviated some objectionable features of Kohlschütter's work. Their criterion of awakening was a predetermined signal.

Michelson (29) was able to keep his sleeping chamber dark, since the selection and the determination of his stimuli were controlled from another room. His criterion of awakening was the pressing of a key, above the patient's head, so that the patient had to raise himself up in order to reach it. This key operated an electromagnetic signal in the experimenter's room. The patient was instructed to give a special signal in case his response was delayed after awakening. Michelson presents the mean of the minimum effective and the maximum ineffective values, where both were obtained.

Czerny (5), as was mentioned above, did not measure thresholds, but presents only the minimum values which he found to be effective. His criterion was variable. He accepted outcries, weeping, assertions like "*it hurts*," or "*it sticks*," facial contortions, visual fixation of the experimenter, and changes of bodily position. The conditions of the experiment required the room to be lighted sufficiently to enable him to see the child. He awoke his patients as often as ten times in one night, at intervals of one hour, if the patients slept so long.

De Sanctis and Neyroz (34) did not measure thresholds, but presented two sets of readings on the scale of the æsthesiometer, corresponding to the two types of response mentioned above. They read the scale by means of a small spot of light projected upon the scale, and upon the forehead but not upon the eyes of the sleeper. They aroused the patient not more than twice in one night. When he awoke, the experimenter questioned him concerning his dreams. It sometimes happened that the maximum pressure which could be

read on the scale did not produce awakening. In such an event, the instrument was withdrawn and the stimulation repeated. The sum of these readings was then taken as the effective intensity: on what ground we were unable to make out from the report, nor did we succeed in imagining any which were valid.

The foregoing discussion will indicate that the series of numbers which the authors present, do not, in any case, express the threshold stimulus-intensities, or even a measure of the intensities whether liminal or not. It is rather difficult, in fact, to assign any meaning to them which one may regard as certain. Nevertheless, when considered as functions of time, some of them present an interesting correspondence with data which are interpretable and reliable; so that one cannot regard the correspondence as accidental.

Kohlschütter (20) presents a curve obtained on one subject covering a series of eight experimental nights, and based on a total of 74 determinations. It shows that the height from which the pendulum-hammer had to fall in order to elicit the required signal from the subject, varied according to the time which elapsed after falling asleep. It increased very rapidly until the end of the first hour, after which, according to the curve, it declines after the manner of a logarithmic curve of decay.⁹ The ordinates of this curve are labelled *Festigkeit des Schlafes*, an expression which he uses interchangeably with *Schlaf tiefe*, and which is therefore frequently rendered "depth of sleep." Three interesting points have to be considered in evaluating this curve.

First, both he and later writers have treated the curve as if it represented the course of "general" sleep; whereas all that Kohlschütter attempted to measure was the course of the depth of sleep with respect to a particular sound stimulus. In fact, he explicitly denies, in another place, that all functions are proportionately depressed in the same epoch of "sleep."

Secondly, if one regards sleep as the product of two dimensions, namely intensity I and duration T , and accepts this curve as showing, ideally, the distribution of the intensity of sleep, some important consequences follow. The quantity taken between any two temporal instants t and t' is therefore expressed by the product-sum $\sum_t \Delta I \cdot \Delta T$. An approximate integration, effected by a graphical

⁹ The general formula for the descending limb of this curve is $y = pe^{x/x-a}$. It is incorrectly given by Kohlschütter (20, 248); probably through a printer's error, the exponent of e is shown as a coefficient.

method, of the values given, shows that about 55 per cent of the total quantity of "sleep" is taken within the first hour, about 82 per cent during the first $1\frac{1}{2}$ hours, and about 92 per cent within the first two hours. This circumstance alone should have sufficed to raise the question whether the kind of "sleep" which is defined in these terms and represented by this curve is the same kind which Kohlschütter really set out to measure. The point seems almost to have escaped mention: one author, indeed, remarked that the recuperative effects of sleep do not seem to be proportional to the quantity taken, but usually this presentation is taken at face value. Our survey of popular as well as of scientific literature disclosed frequent instances in which some eminent authority increased his eminence by advising the public to take its sleep in broken doses, not exceeding two hours in extent. Wherever we could find an allusion to a factual warrant for such advice, it appeared to lie in a very casual consideration of Kohlschütter's curve.

Finally, this curve fits nicely the description given by Fechner (11, II, 33) of the fluctuation of "general consciousness" during this part of the 24-hour cycle. This description is not based on any measurements, but is rather fanciful. As a matter of fact, however, the descending limb of Kohlschütter's curve does not express the numbers in the corresponding table at all; for he obtained numerous readings after the first two hours which are higher than any point on this branch of the curve, some of them being about as high as his first maximum. All these readings, which constitute over 40 per cent of the total number, he rejects. In some cases he gives no reason at all, but in over half the cases he assigns the following reason: namely, that the patient had "*recently*" stirred, so that the measurement was made shortly after a secondary *Einschlafen*, and therefore represented a point near the beginning of a new curve. His criterion of recency was fairly liberal, including half an hour at least, and there is also some reason for suspecting that it may have been elastic. The unpublished experimental results of Johnson and Weigand show that in a typical subject, about one-third of the total time spent in bed is interrupted by stirring every ten minutes or oftener, and about two-thirds of it is interrupted by stirring every 30 minutes or oftener. If Kohlschütter's patient slept much like this one, he could have rejected from two to three times as many measurements as he did reject on the ground assigned. One therefore suspects that if the result conformed to expectation, he retained it

despite the recency of stirring, arguing, perhaps, that the influence of the stir had worn off. If he had considered all his measurements, the descending limb of the curve would have shown several peaks, some of which would have been quite high.

This sort of thing is in fact what Michelson and de Sanctis and Neyroz found: large fluctuations, occurring in an order which is almost rhythmic. They also characterize the curves obtained by Howell (14), which show the temporal changes in the volume of the arm during the time devoted to sleep.¹⁰ They also characterize the curves showing the distribution of motility over the time spent in bed, as obtained by Johnson and Weigand on some 75 adults and by C. R. Garvey on 12 young children, in the course of the Simmons investigation and the studies at the University of Minnesota. Most of these patients were studied over a period of several months, and most of the curves exhibit the result of the examination of several thousand items in the record of the individual subject. While the data of observation in these four experiments were different, they appear in each case as similar functions of time. The tendency or disposition toward immotility, as Szymansky calls it, follows a characteristic rhythm; so, apparently, does the sensitivity of the subject to particular stimuli; so does the relaxation of skeletal musculature. It remains for simultaneous observations to determine whether they all go together, as would appear from *a priori* considerations.

Kohlschütter's elimination of the measurements which showed this tendency therefore seems unwarranted. If the "sleep" which he studied was found to be subject to interruptions and to fluctuations, an "ideal" curve which does not exhibit the fact is not, after all, ideal.

Mönninghof and Piesbergen's (30) curves, showing results obtained on each other, do not show this rhythmic tendency, although all of them have at least two maxima. The first maximum occurs later than Kohlschütter's, as does also the first maximum of Michelson's (29), and of Lambranzi's (25), as presented by Piéron,

¹⁰ These fluctuations he believes to be dependent mainly on the volume of blood in the arm, which he further assumes to represent the complement of the volume in the brain. The *empirical findings* should be distinguished from the interpretation, since the latter is inconsistent with some experimental results obtained since that time. The increased volume of the arm very probably accompanies *muscular relaxation*; the diminished volume, *muscular tension*; so that they may be important elements in the syndrome of sleep, taken by themselves.

the original of which we have not yet been able to obtain. Much discussion has been expended on these divergences. They may be partly due to the diversity of methods employed, and partly to the uncertainties of measurement; but they are not huge as compared with individual differences which were discovered in the Simmons investigation under conditions to which these suggestions are much less applicable.

Czerny's results (5) also show two maxima, one in the earlier part of the night, and one in the later. They are separated by a minimum, in which the sensitivity of the waking state is very closely approximated. The results show no evidence of rhythmic fluctuations of irritability, which is a striking characteristic of the results being obtained on the Minnesota children.

In one respect all these reports agree among themselves: all of them show that it is more difficult to awaken the subject in the period between three-quarters of an hour and two hours after his falling asleep, than at any other time. This finding corresponds to Howell's report of his own observations, and save for some individual differences, it is rather well supported by the results of the Simmons investigators.

Certain other findings of these authors will now be listed, with the qualification that they are rather suggested than proved to hold for "*general*" sleep.

Czerny found (5) that the periods of low irritability of children generally fell in the first and the fourth quarters of the night. This finding suggests that their rest is disposed to occur in two distinct segments. The interpretation suggests itself, that the heightened irritability in the middle of the night is a degraded tendency to awaken for midnight feeding: a habit set up during the earliest weeks of infancy, which persisted for a long time thereafter. Michelson (29) found a similar trend in one individual who was mildly asthenic. Karger (16, 502, ref. 10) interprets such behavior as a sign of a neurotic disposition. Concerning this interpretation, the much more voluminous results of Mr. Garvey on young children, and of Dr. Wholey and Mr. Weigand on schizophrenics, should have an important bearing, when published.

Michelson (29), who found that two healthy, robust subjects showed a minimum irritability (with respect to his experimental stimulus) early in the night, and a heightened irritability in the later part, while the asthenic subject delayed his period of minimum

irritability until the last third of the night, remarks that the disorders of sleep, found in certain diseases, may demand quite different soporific agents according to their type. In senility and in the manic phase of manic-depressive insanity, so he asserts, the patient quickly falls asleep, but awakens very early, and has great difficulty in falling asleep again. In such disorders, a soporific is indicated whose action is delayed for some hours after administration. He suggests the use of sulphonal. In asthenias and in the depressive phase of manic-depressive insanity, the patient, so he says, tends to remain restless and irritable until late in the night, the deepest rest occurring toward morning. Such behavior, he considers, would indicate the use of a quickly acting drug, such as paraldehyde. This problem is of first importance, but we consider the method of study inferior to that of registration of motility, combined with statistical description. We shall have more to say concerning it in our discussion of Guttman's (12) report.

Following a dose of paraldehyde, Michelson found that the irritability to his experimental noise was greatly decreased. The effect set in quickly, and persisted for $2\frac{1}{2}$ hours. The experiment was made in the afternoon. He also found that his patients were much more easily awakened by the experimental noises in the spring and summer than in the fall and winter. This finding corresponds to popular belief, and is corroborated by the studies on college men (17) made at Mellon Institute.

Czerny (5) compared records made on children during nights which followed an afternoon nap with records made during nights which did not. Following the nap, the first minimum of irritability to electrical stimulation is slightly postponed; but during the greater part of the night, the patient is harder to awaken than when the nap was omitted. The finding suggests that within limits, the more time the patient devotes to resting, the more quietly will he tend to rest while he is about it. Such an interpretation will shortly receive independent support.

Kohlschütter (20), Mönninghof and Piesbergen (30), and Michelson (29) report that they found it harder to awaken a patient after he had recently stirred and relapsed into quiet, than at ordinary times. It seems probable that certain stimuli produced in the course of the maintenance of posture itself may serve to reinforce the experimental agent of awakening. Among them are stasis of blood and other bodily fluids, interference with circulation, overheating of the

unventilated portions of the skin, the stretching of muscles, the cramping of joints, and the pressure of bed-clothing upon the body, the body upon the mattress, and one visceral organ upon another. In time these conditions must become irritating, and eventually lead to a change of bodily position, by which they are relieved. Shortly before the patient stirs, external stimuli are reinforced by them; for a short time thereafter, a great deal of internal stimulation is removed, so that the external agents must be abnormally increased to produce a given response.

Mönninghof and Piesbergen (30) report that when the experimental night is preceded by unusual exercise, such as long walks, after which a little alcohol (four glasses, or less, of light beer) is taken, the subject became more sensitive than normal with respect to the experimental noise. When exercise was omitted, and much more alcohol taken, the subject became less sensitive to the noise during the earlier portions of the night. They attribute the latter circumstance partly to the fact that the patient stirred more often than usual, so that more of the measurements were made shortly after he had settled than would normally be the case. One would suspect, however, that the increased frequency of stirring might be due to the presence of extraordinary irritation—perhaps in the alimentary tract, the bladder, etc., which would operate as a reinforcing agent unless there was a concurrent reduction of *specificity* of response. We are not well satisfied, therefore, with the explanation proposed. On the factual side, however, we can add a little in corroboration: we have direct evidence that a sleeper stirs considerably more frequently than normal if he is drunk when he goes to bed.

2. *Studies based on registration of changes in bodily position*

The pioneer in the use of this method appears to have been Szymansky (36, 37, 38), whose investigations were begun prior to 1914. The results, of course, are strictly relative to the method, and are meaningless unless the principles and the limitations of the latter are considered along with them.

The essential requirements of obtaining the original data are extremely simple, although Szymansky, as well as Karger (16, 502, ref. 11), Naegele (16, 502, ref. 10), and others, exhibited great ingenuity in designing and constructing their experimental apparatus. The cage, or bed, is mounted in such wise that it yields to the movements of the occupant. It is then connected mechanically or pneu-

matically to a recording instrument, which registers some, though not all, of the components of the movement on a strip of moving paper. (The Simmons investigators have since found that electrical transmission is also feasible.) If the speed of the paper is uniform, the time at which the subject moves may be obtained directly by scaling: otherwise, an independent time-record is necessary. In general, the displacement of the recording pen is not proportional to the work performed in effecting the change of bodily position. This assertion is based on the fact that all the mountings were subject to periods of their own, so that the displacement of the pen depended in part on the phase in which the impulse was given to the mounting (a single stir may impart several distinct impulses, whose temporal separations are unrelated to the natural period of the mounting); and on the further fact that at least one component of the reaction of the mounting is left unrecorded. Such being the case, an attempt to convert the magnitude of the displacement of the recording pen into an expression of the energy expended by the subject in performing the change of bodily position, is absurd.

Some displacement must therefore be chosen as the minimum to be considered. Inasmuch as instruments vary in sensitivity and in magnification, it should first be decided how much of a bodily stir is worth considering, for the purposes of the experiment, and then find the least displacement which such a stir will produce, for the subject under consideration, and treat such a displacement as the smallest which will be taken into account. It is easy enough to make an apparatus which will record the tremor of an eye-lid: Szymansky (38) made a heavy platform, hung from an overhanging beam, with brushes dragging on the floor to dampen its oscillations; it carried an easy-chair on which the subject sat. The whole system would seem to be clumsy enough, but by employing pneumatic transmission and a sensitive tambour, he found that it registered the respiratory changes in blood-pressure as well as changes of bodily position, so that he had to reduce its sensitivity to get a record which he cared to interpret. Most of the continental investigators who have worked on human subjects seem to consider the movement of a limb to be the smallest stir which is worthy of being taken into account.

The judgment which the record permits is therefore categorical: within a given temporal extent, the patient either made a stir which exceeded the minimum displacement, or else he did not. There is

no possibility of gradation of activity except in some statistical manner. The choice of a suitable procedure presents a problem, which one of the present writers (J.) has attacked as follows:

Assemble the records made on a single subject during a considerable number of experimental nights (twenty nightly records made under comparable conditions, is somewhere near a satisfactory minimum). Divide each nightly record into convenient segments: five minutes is a convenient length, as it is about 1 per cent of a fairly typical stay in bed ($8\frac{1}{3}$ hours). Mark each segment as "active" or "passive," according as the minimum displacement is exceeded or not. Choose some one of these intervals as a "zero" interval, which is made constant for every experimental night: one may choose, for example, the interval which begins at 11:15 P.M., or (disregarding the time as shown by the clock) the interval at which the subject retired, for the zero. Give each interval a number, in order, from the right or the left of the zero-interval. Next, count the number of experimental nights in which each of these intervals was found to be "active," divide the result of the count by the total number of experimental nights, and plot the quotient against the interval-number. The result expresses the relative frequency of movement, obtained by actual count, for each interval, taken over the number of experimental nights, and therefore the relative *probability* of movement within the corresponding interval on the fictitious, average, statistical night that is best represented by the empirical data. The construct is, of course, a fiction, in the sense in which every average is a fiction, and in the same degree. It nevertheless permits certain comparisons which otherwise could not be made: namely, of *systematic* effects of drugs, or of the season, or of bedding equipment, or of daily activities, or of a change of phase of a cyclical disease or mental disorder. Its use is almost necessitated by the fact that no direct means exists of measuring a *tendency* toward motility.

When we turn to the manner in which the continental investigators handled their data after they had obtained them, we find occasion for disappointment. Szymansky (38) obtained but *one record* in the case of each of his ten subjects, five of whom were men and five women, aged twenty to thirty years, all being healthy. He kept each of them in bed for twenty-four hours, and instructed them to remain as quiet and passive as possible. He permitted "indifferent" but not "exciting" material to be read. Eight of the subjects obeyed his instructions.

From simple inspection of the records, accompanied by a rather indefinitely grounded estimate of the amplitude and the frequency of jags in the record-line, he assigned each stretch of an hour, or less, to one of four categories: namely, "absolute rest," "relative quiet," "abated motility," and "active motility." The boundaries are rather vague, especially between the second and the third. The first two classes he interprets as indicative of "sleep," the basis being the results of interrogation of the subject the next day.

We shall not discuss Szymansky's interpretation of his results in any great detail, for the flexibility and the uncertainty of his statistical procedure prohibit a definite interpretation. Moreover, we must add that our own experiments have shown that large variations in the manner of resting of an individual subject from day to day are the rule, not the exception. To indicate what variability may be expected from this cause, we may suggest that if the total number of five-minute intervals during which the patient is in bed be indicated by the symbol T , while the number of these intervals in which the patient registered one or more changes of position as great as the movement of a limb is denoted by the symbol t ; the ratio t/T , which can be regarded as an expression of relative motility, varies through a range of 50 to 80 per cent, according to the subject, from night to night. Consequently, a single night's record, such as Szymansky gives, affords no reliable representation of the patient's *habitual* manner of resting: neither does a single pair of records, such as Karger (16, 502, ref. 11) tenders, afford a reliable means of comparing the influence of an experimental variable (a drug, or a fairy-story, etc.) with a stable norm. An interpretation of such meager evidence is little more than a guess. This judgment must hold, despite the fact that both of these authors guessed very astutely, on some occasions, as we shall presently see.

On the basis of single records, and a rather free interpretation thereof, Szymansky assigns his ten adult sleepers to three typical classes. The first type, so he informs us, shows uninterrupted "absolute rest" from the time they fall asleep until after midnight. After midnight they show "absolute rest" interrupted by three or four periods of "relative quiet." Five of his ten subjects conform to this type.

A second type showed "absolute rest" during the whole night with only one brief period of relative or active motility, which

involved the satisfaction of some "physiological need" (excretion?). Three subjects were said to conform to this type.¹¹

A third type is represented by two individuals, who showed five to seven periods of "relative quiet" or of "abated motility," whose total duration exceeded that of "absolute rest." He regards such a type as abnormal. One of the two subjects had suffered from toothache, and the other, contrary to instructions, had read an exciting story for two hours just before going to sleep.

It seems to be clear that by a period of "absolute rest," Szymansky does not mean a stretch of time during which the patient makes no major changes of position whatever, but only occasional, widely separated, changes. It is not clear how often these stirs must occur to cease being called "occasional." If one called "absolute rest" unbroken only while major stirs did not appear, then none of the 90 subjects thus far employed in the Simmons investigation could be classified under Type I or Type II. One subject, an insane woman, could have been considered under Type II during some three nights when she was heavily drugged, but even those records were not typical of her habitual manner of resting. All the others would belong to Type III, with huge individual variations within that type. The difference does not rest upon differences in the sensitivities of Szymansky's instruments and ours, but in the manner of taking account of what the records show.

Szymansky was not less interested in the distribution of motility over the day than in its distribution through the night. Of the time not devoted to sleep, the eight subjects who obeyed instructions spent from 56 to 67 per cent of the day in "active motility," the average being about 62 per cent. The main periods of motility came during the forenoon and late in the afternoon, thereby reminding Szymansky of a finding of W. Stern, reported in *Ueber Psychologie der individuellen Differenzen* in 1900. At different times of the day, Stern had made his subjects tap out a rhythm in waltz time, and had plotted the tempo against the time of day. The curve resembled a wide latin "M" in that it was low at the beginning of the day, rose to a maximum during the forenoon, declined until one or two hours after the midday meal, rose to a second maximum late in the afternoon, and again declined toward evening.

¹¹ One such record (schematized) appears in 38, 200, Fig. 3, Chart II. The description, given on page 203, does not fit the record as given, and indicates that the latter is inverted, so that 12 o'clock noon appears as midnight.

We now encounter an interesting argument, which has the form of a true inference, but the conclusion appears to result from a change of name. Szymansky renames the periods of "active motility," calling them periods of a "*disposition toward labor*." He then remarks that the organism can work (expend energy in *specific* responses?) more readily when it is already disposed to work than at other times. Since by assumption the periods of frequent squirming express a disposition toward labor, it follows that the times of day when they appear are the ones during which one can labor the most economically. Considered as a model of inductive reasoning the argument is capable of much improvement, as is also the interpretation which Stern put upon his own results. Nevertheless, the measurements made by Wyatt and Fraser (reviewed in 16) on the output by half-hourly periods of women factory workers on one operation conform rather well to the expectations raised by both of these authors.

Szymansky's earlier work (36) on infants indicated a half dozen periods of "absolute rest," and as many of "active motility" in the 24-hour cycle. The differences between this distribution and that of the adult he attributes in part to the change in the metabolic rate, and partly to the increasing influence of light as early infancy is passed. The motile periods of the infant tend to merge as age progresses, until, in early childhood, it may be considered "monophasic" if the afternoon nap is omitted.

Kreidl and Herz (22) obtained one 24-hour record on each of five blind subjects, together with ten nocturnal records on one of them. They also obtained one record on each of two deaf subjects, two records on two others, four on still another, and five on one besides. They used two subjects who were both blind and deaf, obtaining ten records on one and five on another. Of this collection of data, two sets—*i.e.*, those which contained as many as ten records on one subject, would justify the kind of statistical treatment which we suggested above. We venture the hope that they will receive it before they are destroyed, for the problem is important. The authors simply give verbal descriptions of the movement-records taken singly, night by night. Their findings may be summarized in a word: the descriptions would fit the records of our most typical *normal* subjects, and they also conform well to the similar descriptions of the normal subjects employed by the authors.

These results should be considered in relation to the classical

report of Strümpell, cited by the authors. Strümpell's patient was a neurotic youth, who was said to be completely anaesthetic except for impressions made on one eye and one ear. If these channels were shielded, it is said that the patient promptly "fell asleep."

It strikes us that the diagnosis of "complete anaesthesia" to stimuli applied elsewhere has been accepted rather meekly. Was the "muscle-sense" completely lacking, for example? If it was, it is hard to see how the patient could perform enough coördinated movements to live. In *locomotor ataxia* one has a virtual suppression of the muscular sense in a restricted region: what would happen if such disorganization were general? On the other hand, if this kind of sensitivity remained, the patient would not be removed from stimulation when vision and hearing were depressed. The suspicion is growing that the greater portion of the nervous current used in effecting response may be generated in muscle-spindles and in undifferentiated tissue and free-nerve endings, by the organism's own responses. If this view is tolerably correct, the classical interpretation of Strümpell's case breaks down. It has been previously suspected, of course, that the patient was shamming, after the manner of hysterics. Kreidl and Herz have tended to confirm the interpretation that the most important stimuli to waking are applied within and upon the patient's skin. Once they are effective, ordinary lights and sounds may interfere with a return to sleep; but taken by themselves they are not important disturbers.

Guttmann (12) obtained over 100 separate records, of which about half were yielded by manic-depressives. A part of his problem consisted in ascertaining what soporific agents were indicated, and when. He remarks, judiciously, that both their administration and the evaluation of their effects has rested mainly on guess-work hitherto, and that the motility-record offers a possible means of forming a more reliable judgment. One would think that the first necessary step is to determine what is the normal manner of resting of each individual patient. As was mentioned above, a person in robust health varies markedly from night to night with respect to his average. The effect of a soporific, therefore, cannot well be evaluated unless a considerable number of comparison-records are available, unless it produces a huge deviation from the normal which can be shown to be relatively improbable by the application of the laws of chance. Furthermore, it has developed in the Simmons Investigation (to be elaborated upon in a forthcoming monograph) that each individual has his own norm. The fraction of the time spent in bed

which a healthy individual spends in immotility varies from person to person through a range of more than four to one. It is therefore unsafe to assume that a given person should habitually spend any particular proportion of his time in bed, in quietude. Some such assumption appears to underlie Guttman's procedure. Certainly such records as Nos. 9 and 10c (p. 318) and 14a (p. 323) show no greater frequency of movement than the most typical records of some of our most representative subjects in perfect health: yet it was considered that veronal and luminal were indicated in the first case, veronal in the second, and tincture of opium in the third. Let us hope that the basis of such an interpretation lay outside the motility-records for none appears there.

Guttman, as well as Karger (19b) has produced some very suggestive evidence concerning the action of certain hypnotic agents. The influence of veronal and of luminal seems to be much more pronounced on the second night following its administration than on the first. The growing tendency toward household use of derivatives of barbituric acid would seem to warrant an inquiry into the persistence of the depressive effect through the next two working days, as well as through the two nights following the dose. If the persistence which these investigators appear to have discovered should prove to be general, it would seem unnecessary to administer such drugs as often as once in 24 hours, in order to obtain the sedative effect. Chloral hydrate (19b) appears to have increased motility on the first night following its administration; sodium bromide seems to have been promptly effective in the case of one child who had proved resistant to chloral hydrate and luminal. The combination of opium with veronal (12) was followed by complete immotility which endured for several hours: a condition, let us add, which almost never appears in the healthy subject in ordinary circumstances.

The time would now appear to be ripe for some intensive, well controlled experiments on the problems of soporifics which these experimenters have so well set. The method appears to be adequate if the results are properly scored and treated, and if a sufficient mass of them is accumulated to permit chance variations to cancel each other.

Szymansky (38) also required 16 subjects to spend 12 to 30 minutes in an easy chair, mounted on a yielding platform¹² to which

¹² This apparatus, though simpler than that of Renshaw and Weiss 33-a is capable of indefinitely greater sensitivity than the latter, and has much to commend it.

a recording instrument was attached. He instructed them to assume a comfortable bodily position, and to refrain from bodily movement as far as they possibly could, for the duration of the experiment. Fifteen of these subjects stirred, in the average, once in 1.2 minutes, the range of variation lying between 0.6 and 3 minutes. An additional subject appears to belong to a different class: she made but three stirs in the whole 30 minutes. If her record be averaged with the other 15, the average length of the "rest-period" for the whole group is 1.8 minutes. This is about the same average as a group of tubercular women, observed during the Simmons investigation, showed during the hourly periods in the forenoon and the afternoon when they were forbidden to sit up in bed, to talk, read, write or primp. During the remainder of the day they were still more active.

Szymansky reasoned that his subjects, in order to refrain from stirring, had to exercise volition: that the longer they maintained this form of self-control, the more will-power they had to exercise. He suggested that the measure of performance in this experimental task, under the experimental instructions, might be taken as a measure of patience, persistence, or will-power. Aside from the fact that different people may be differently irritated by stimuli which arise from a long maintenance of bodily position, a still more important question ought to be considered. Granting, as we must, that this experiment yields a direct measure (in minutes) of the subject's persistence in sitting still despite the presence of an unmeasured and unanalyzed complex of irritation, does it therefore enable us to predict how long he will persist in some other kind of performance, despite the presence of other kinds of irritation? If so, how was the fact established? By independent measurements of *both* performances, or by the Method of Proclamation—the reliance of the magician¹⁸ of the past and the mental tester of today?

Van Leeuwen (27) studied the nocturnal activity of a dog under four conditions: namely, normal, dosed with caffeine, dosed with extract of ordinary coffee, and dosed with extract of coffee from which nearly all the caffeine had been removed. His apparatus was somewhat like one of Szymansky's: the animal's cage was suspended on helical springs, with a rigid connection to a corrugated rubber tube, closed at one end, and connected at the other with a Marey capsule,

¹⁸ For an interesting example, *cf.* the 'test' of guilt to be applied to a wife whose husband, without evidence and without confession, accuses her of adultery, in the *Law of Jealousy*, Numbers v.

the rubber diaphragm of which supported a writing pen of a slow moving kymograph. By means of a ratchet-pawl device, the vertical displacements of the cage were made to actuate a rotation-counter. Thus the total displacement which occurred within a given time could be obtained by comparison of the two readings. The author presents some sample kymographic records and a table of differential readings on the integrator just described: one reading for each night. Because of the manner in which the results have been exploited in propaganda for decaffeinated coffee, they invite special attention here.

The dog weighed 6 kilograms ($=13.2$ pounds), which is roughly one-twelfth the weight of an average man. Hence, if the doses here employed are multiplied by 12, the product will express the equivalent dose relative to body weight for a man who weighs 72 kilograms, or 159 pounds.

On the nights in which no doses were given, the rotation-counter gave a maximum reading of 48, a minimum of 13, and an average of 30.5 revolutions. On a night following the administration of the extract of 16 grams of ground coffee-bean the number of rotations registered was 98.¹⁴ This dose, which, according to the author, is the smallest one whose effect can be reliably shown, is the equivalent of eight cups of strong coffee for a man weighing 72 kilograms, or 159 pounds. (The author considers 25 grams of ground bean to the cup as being strong.) This record is compared with a so-called normal record, in which the displacements of the cage produced but 31 rotations of the integrator. On their face these two records make good propaganda, provided one assumes to begin with that the dog ought to spend 14 hours of the night in resting as quietly as possible. The records made on the kymograph tend to abate the impression produced by the other readings, by giving additional information. On the night following the administration of the coffee extract, the dog showed a half hour of almost continuous locomotor activity between 5:45 and 6:15 P.M., with occasional revivals for five minutes or less at a stretch until 9:45 P.M. These periods contained nearly all the excess of activity over the normal night, although there are two other brief periods of activity: one about 12:20 A.M., and the other about 6:30 A.M. Both of the latter were followed by rest which appears to be about as quiet as that shown in the "normal" record. Whatever injurious effects may have resulted from this huge dose,

¹⁴ On another similar experimental night the same dose was followed by a reading of 70, a reduction of some 29 per cent.

none appear in the dog's motility record. In fact, the latter shows nothing save an effect which people often drink coffee in the hope of getting: namely, an evening of heightened activity, followed by a night of quiet rest.

Van Leeuwen also shows a kymographic record following the administration of the extract of 80 grams decaffeinated coffee. The integrator registered but 55 rotations: and yet, between the hours of midnight and 6 A.M. there appears much more activity, both postural and locomotor, than in the normal record or in the record which followed ingestion of the extract of 16 grams of coffee.

In a propaganda booklet (28) distributed by the vendors of one brand of caffeine-free coffee, we find some of the foregoing results, along with others in the same article (27), which is specifically mentioned; but the results are expressed only in terms of the total number of rotations of the integrating wheel, thus: Normal, 48; after 25 g. coffee, 148; after 80 g. caffeine-free coffee, 55. The figures are taken from van Leeuwen's table, but in his original report he does not give the kymographic records for the first two experiments just mentioned. We have discussed the record belonging to the third. If the distribution of activity according to the time of night be taken into account, it is extremely difficult to see how such data as these can lend logical support to an argument in favor of caffeine-free coffee.

Here we must mention that on page 7 of 28 there appears a drawing of van Leeuwen's apparatus beneath the following legend: *Verscheuchung des Schlafes durch coffein. Versuch von Prof. Dr. W. Storm van Leeuwen (Universität zu Leiden). Studien über die Wirkung von Coffein, Kaffee und coffeinfreiem Kaffee.* Two curves are shown, which resemble kymographic tracings. One is labelled *Normal*: the other, *Diese automatisch registrierte Kurve zeigt die nächtliche Bewegungsunruhe des Hundes nach Coffein.* Neither curve resembles any portion of the ones which van Leeuwen publishes in 27, and neither, in our opinion, could have been made with the recording instrument which he used.

More recently another manufacturer has reproduced (39) portions of these two charts, again referring to van Leeuwen's report, but omitting the picture of the dog and the apparatus, and giving no hint that the records and the doses pertained to a 13-pound dog rather than to a typical human patient, for whom the preparation was recom-

mended. If Dr. van Leeuwen is not a party to this deceit, we suggest that a disclaimer from him would be appropriate.

To return to the original report (27). A kymographic record is also shown of a night's activity following the ingestion of 0.2 grams caffein sodium benzoate (*ca.* 1 gram caffein). According to van Leeuwen's citation of Lehmann, this quantity of caffein represents the quantity extracted in ordinary cooking from about 16 grams of coffee. Taking 25 grams of coffee bean as representing one strong cup, this dose represents the equivalent, relative to body weight, of eight cups of strong coffee for a 72 kg. man. The reading on the rotation-counter is not given, but the kymographic record shows an abnormal amount of locomotor activity until about 12:30 A.M., followed by rest which is distinctly more quiet than that shown in the "normal" sample-record, and which remains so until after 8 A.M. We do not presume to prescribe the hours at which a dog ought to take his rest, but if only such results as are shown are to be considered, and if they are applicable to the normal human subject, this exhibit could serve as a basis of propaganda against the removal of caffein from coffee: the dog, under this relatively huge dose, did exactly what normal people hope to do after drinking coffee in the evening.¹⁵

Certain other interesting comparisons appear in van Leeuwen's table. The integrated activity, indicated by the reading on the rotation-counter, does not increase regularly with the dose. The extract of 50 grams of coffee was followed by 112 rotations: the extract of 25 grams, administered two nights later, was followed by 148, or 32 per cent more than that which followed a dose twice its size. The most quiet night reported followed the administration of 6 mg. caffein by mouth; only 12 rotations occurred. Relative to body-weight, this dose would represent the caffein-content of half a cup of strong coffee administered to a 159 pound man. On the whole, we suspect that the results of this experiment do not indicate as much of an effect of coffee and of caffein on the sleep of the human as they actually have. The assumption is implicit that the effect on the dog indicates the effect on man: as far as the motility-records go, no evil effects are shown.

¹⁵ We hold no brief for the use of coffee; neither have we any fault to find with either of these advertised preparations, as such. We protest, however, that van Leeuwen's findings are irrelevant to the propaganda based ostensibly on them. No light whatever is shed on the relation between coffee and rest by the results cited.

3. Studies which employed measurement of oxygen-consumption

Laird and Wheeler (24) describe a method, invented by themselves, of measuring what they call "mental efficiency in the true sense." They applied it to the effect of curtailing the subjects' stay in bed from eight hours to six, accomplished by postponing the time of going to bed. The logical foundation of the method requires consideration before the results.

On awakening in the morning, and while still lying in bed, the subject is connected, by means of a modified gas-mask and tubing, to a gas-receiving apparatus. If no accidents, such as leakages, occur, analysis of the collected air permits determination of the quantity Q of oxygen (measured in cubic centimeters or in grams) which the patient consumed during the test. If the weight or volume be multiplied by the proper factor (which happens not to be perfectly constant for all conditions), Q may be expressed by its heat-equivalent, in calories; and subject to certain fairly plausible assumptions, it will represent the energy converted into heat in the course of the bodily activities during the time in question. If the time T required to collect the sample be measured in hours, the ratio Q/T expresses the oxygen-consumption in (say) grams per hour, or the rate of heat-production in calories per hour during the time T . This is the temporal rate of output for the total body surface: if the latter varies from time to time, as it may, or from person to person, as it does, it should be divided into Q/T to reduce the comparisons to a common basis. In the present experiment, this was not done, and was perhaps unnecessary as one person was not compared with another. Since this determination of Q/T was made while the patient was lying still, and under instructions to remain idle, it is called a "resting" rate of metabolism; and being taken as *standard* for the day, will hereafter be denoted by Q_0/T .

During the remainder of the experiment the patient continues to lie in bed, but he is now given a list of problems in multiplication which he is to solve "mentally." After ten minutes of such work he is again connected with the respiration-apparatus, and another sample of air is taken during five minutes of continued multiplication. The rate of "working" metabolism may be derived from this analysis, and expressed simply by Q/T .

The expression $\frac{Q - Q_0}{T}$ gives, in itself, merely a comparison

of the rates at which the subject takes oxygen from the air (or at which he generates heat) at two different times. The authors put an interpretation on it which we shall consider later, but we shall first dispose of the remaining formal considerations.

The subject is required to solve n problems at each experimental sitting: in the present case $n=15$. The time T which he spent in solving them was measured in seconds, and may conveniently be expressed in hours. The expression n/T gives the rate of multiplication: if errors are to be considered, n in the expression may be reduced proportionally to the number of errors, or T increased by an amount proportional to the time consumed in making them. This correction, however, is a detail.

Let us now define a formal relationship:

$$R \equiv \frac{kn/T}{(Q - Q_0)/T}$$

which instantly reduces to

$$R \equiv \frac{kn}{Q - Q_0}$$

The factor k is a proportionality-constant, whose value depends on the units employed for measuring Q and T respectively. If one value be assigned to k , then R expresses *problems per liter*; if an alternative value is assigned, R expresses *problems per calory* (of generated heat). The expression implies *no physical relationship whatever* between problems and calories. This intrinsic lack of meaning must be remembered.

The value of R may be determined under two sets of conditions 1 and 2: an absolute comparison is given by $R_1 - R_2$ and a relative comparison by $(R_1 - R_2)/R_1$. In the present experiment, condition 1 is especially distinguished by the fact that it was preceded by a stay of eight hours in bed; condition 2 by a stay of only six hours. Each condition was studied for one week, condition 1 being taken first. Three subjects were employed, all of whom had undergone "several weeks" of practice in the arithmetic operation, and were showing "no further improvement."¹⁶

The authors do not present a formal statement of the values for

¹⁶ All the subjects, nevertheless, did give a higher speed in multiplication during the second week of the experiment than during the first, the increase varying between 0.9 and 9 per cent. The authors disregard the possibility of the effect being due to practice; in one place they suggest that it is related

the three subjects of the difference $R_1 - R_2$, but they present the data which would enable such a statement to be made, and also say, in effect, that such a comparison constitutes the crux of their study. We shall now mention their results briefly, and then inquire into their meaning.

For all the subjects R_2 is less than R_1 . Is the difference fairly large when compared with its own probable error? The authors give no measure of variability, so we cannot tell.

Under condition 2, n/T is greater, for every subject, than under condition 1. Each patient multiplied a little faster (by 0.9 to 9 per cent) after an allowance of only six hours for sleeping than after an allowance of eight. *Are these differences statistically reliable?* The authors do not tell us: neither do they offer us a means of judging, as they should have done whether they expressed an opinion or not.

Under condition 2, in the case of two of the subjects, Q_0/T is greater than under condition 1: *i.e.*, the patient generates heat more rapidly after six hours devoted to rest than after eight hours. Probably, as the authors suggest, his muscular tension is higher, on the whole; he is perhaps generally more irritable, more responsive. Such a result is not surprising: but *how does this difference compare with fluctuations from day to day under either condition singly?* We are not informed, although the information would have been interesting whether the authors emphasized the difference or not.

Under both conditions, Q/T is greater than Q_0/T .¹⁷ That is to say, the patient generates heat more rapidly when he is multiplying

than while he is "idle."¹⁸ The difference $\frac{Q}{T} - \frac{Q_0}{T}$ is greater

to the loss of sleep, and in another place deny that loss of sleep affected the "mental output." The largest improvement (9 per cent) ought to be explained somehow.

¹⁷ The authors *assume* that the difference was *necessarily incidental* to the performance of the mental work. They do not mention the earlier study of Benedict and Carpenter, done on 22 subjects, which yielded no differences that the experimenters were willing to attribute to mental work (taking examinations). This result (2) has been considered authoritative. A discussion, or at least mention, of this apparently negative finding would have been proper.

¹⁸ On some days of the experiment, a second set of readings should have been taken without the subject being required to engage in mental multiplication. Such a control would have given an indication of the effect on rate of oxygen-consumption of activities that are incidental to waking up.

under condition 2 than under condition 1: *i.e.*, although the patient is multiplying somewhat more rapidly, he is also generating heat more rapidly, after six hours of rest than after eight, in comparison with the standard adopted for the day. *What is the statistical reliability of this difference?* This information, also, is withheld.

We must now inspect the authors' interpretation of the relationship between problems solved and heat generated which we have denoted by R . The empirical finding that Q/T is greater than Q_0/T means simply that energy was converted into heat more rapidly *while* the patient was doing mental multiplication than while he was not. The authors assert that *all* of this additional expenditure was invested in activities which were *necessary* to the performance of mental multiplication: "A comparison of the working and resting caloric consumption shows how much additional energy is needed to do the mental work." Thus, by assumption, $Q - Q_0$ measures the energy-input of mental work, and n the mental output: whence, $R \equiv kn/(Q - Q_0)$ is a perfect expression of "*mental efficiency in the true sense*," and is shown by experiment to be greater after eight hours of rest than after six.¹⁹

What the authors have done to establish their "true statement of efficiency," is to give a new name to $Q - Q_0$, and then to reason as if their christening had miraculously endowed it with the properties which are suggested by the name. They now call it an expression of energy expended *in* multiplication, instead of additional energy expended *during* multiplication, disregarding the fact that multiplication was accompanied by other activities, not all of which are known to be necessary thereto, which also call for the expenditure of energy in *variable* amounts and rates.

Their interpretation implies that two things can be, and have been, determined: namely (a) what members of the universe of bodily activities occurring at the same time are necessary to mental multiplication, what activities are irrelevant to it or dissociated from it, and what activities are interfering with it; and (b) at what rate energy is being expended in each of these three classes of activity. If this review were intended solely for the technical reader, further comment would be unnecessary; but for the benefit of the non-technical readers, cultural and general, to whom the original report was addressed before it was submitted to the scientific public for

¹⁹ *At the time of day at which the test was made*, of course, and subject to the conditions, described and undescribed, which then held.

inspection and criticism, we may add that Laird and Wheeler made no such determinations; and that the technical methods which are now available in physiology and biophysics are too limited to make such determinations possible. What they tender is a counterfeit method of attacking the problem of "*What it costs to lose sleep*," along with a sham solution, neither of which need to be considered further.

4. *Studies which employed histological examination of tissues.*

Bast and his collaborators deprived rabbits of sleep by placing them in slowly rotating cages, which compelled the animal to take at least one step as often as eight times a minute.²⁰ Thus the animal had to walk, at the least, 1.4 miles a day, but this minimum requirement was not considered excessive. Satisfactory observations were made on 14 animals out of the total number studied. Nine out of the 14 developed characteristic symptoms before death. The other five were killed on the 30th day, without showing characteristic symptoms. Collapse was typically preceded by a sudden fall in temperature, a considerable rise in pulse-rate above its recent level, followed by a sudden fall, and a gradual decline in the rate of breathing. One animal appeared to have attained complete exhaustion at the end of seven days, and another at the end of 17 days; but after five or ten minutes in which there were some convulsive movements, they resumed their former activity without further evidence of fatigue. Only four animals attained complete exhaustion while the observer was present: they showed the following symptoms: body relaxed, but muscles showing fibrillary contractions; eyes half open; corneal reflex absent; marked salivation; sphincters relaxed; average pulse-rate 132 per minute; average respiratory rate 42 per minute; rectal temperature 35.6°C. (= 105.1°F.); handling the animal while in this condition caused convulsions.

Following this seizure, the body completely relaxed, the pulse became irregular and feeble, the temperature dropped rapidly, and the respirations stopped. Heart-beats continued for one to two minutes thereafter. It was impossible to predict with certainty the

²⁰ There appears to be an important difference between the treatment of the animals in this experiment and in that of Crile. In this experiment, the disturbances appear to have been regular, so that the animals had opportunity to learn to take their rest in temporal segments of a few seconds each. It may be that some of them succeeded in making so profound a modification of their normal habits. In Crile's experiments it appears that no such opportunity was given, for the animals were disturbed whenever they slumped down to rest.

onset of complete collapse: for the characteristic symptoms were in many cases repeated several times, to be followed by recovery of temperature, pulse-rate and respiratory rate, which might be retained for several days.

Nervous tissue from sixteen fatigued animals and seven normal animals was subjected to histological study. The technique of preparation was very carefully controlled to avoid spurious effects, such as have been suggested as a possibility in the work of previous authors. The point is emphasized that by ignoring the great variety of appearances of tissue in supposedly normal animals, one may easily draw erroneous conclusions from comparison of experimental animal tissue with that of normal animals.

Their examination of nervous tissue partially confirmed the findings of Crile (4), Hodge and Dolley (cited in 4), all of whom had reported evidence of nerve-cell degeneration in exhausted animals. The present authors found cells in the gray matter, usually in the visceral motor area, which showed partial or complete chromatolysis, although most of the cells, particularly in the somatic area, appeared normal. This was true only of animals which had been carried to complete exhaustion: in others, no deviations from the normal were detected.

Their criterion of chromatolysis was two-fold: granulation of Nissl's bodies, and vacuolization of the cytoplasm. They regarded all other symptoms as unreliable. They observed no shrinkage of the cells or the nucleus, or migration of the nucleus toward the periphery. The nuclei of the cells in the normal animals took a light stain: those of the exhausted animals did not.

Chromatolysis was found in some nerve-cells of normal animals: hence it was only because a definite nuclear change, as well as chromatolysis, was found in the exhausted animals that the authors were willing to attribute degenerative changes to fatigue. Ruptured cell-walls were sometimes found in the vagus. In the medulla, chromatolysis was variable. Characteristically it seemed to begin at the cell wall and progress inward toward the nucleus. In any given area, most of the cells were usually normal in appearance, others being moderately changed and still others almost completely colorless. However, some fields were found in which most of the cells refused to take the stain. Some degeneration was found in the spinal cord, but less than in the medulla.

While their findings, on the whole, were positive, they remark

that "it is not as easy to detect nerve-cell changes following prolonged periods of sleeplessness as one might be led to expect judging from the findings of those who have studied the effect of other forms of fatigue on nerve cells."

5. Theoretical contributions

Carlson (3) emphasizes the importance of muscular relaxation in the production of sleep, suggesting, as one of the present authors (*J*) had done, that muscular tension may provide the principal source of stimulation necessary to normal activity. The same suggestion is proffered by Dunlap ²¹(9), who stresses the disintegration of activities in drowsiness and sleep. Hollingworth (13) treats sleep as a silly, vicious habit, more devastating than alcoholism, opium-addiction, and yellow fever, for which no excuse can be found except that everyone indulges in it. His general viewpoint appears to be essentially that which was held, for a time, by such religious leaders as St. Francis of Assisi, St. Teresa, and the Methodist leaders Fletcher and Wesley; all of whom endeavored also to practice it, but abandoned it in the face of the results of the attempt.

6. Miscellaneous Studies

The work of Landis (26) shows that changes in the electrical resistance of the body are apparently unrelated to sleep and waking, or to "mental activity" while the subject is awake. He attributes the results found by Richter (16, 502, ref. 20) to changes in the conditions of the electrodes (which occur without the patient's body being included in the circuit), to variations in intimacy of contact, and to a counter electromotive force set up in the body in opposition to the polarizing current. Only one-fifth of the total obstruction offered to the latter is found in the skin;—not all, as Richter had claimed. The work of Darrow (7) indicates that Landis is partly wrong: that the varying resistance to the external current is probably due, almost altogether, to the activity of the sweat-glands. This interpretation is strengthened by the proof, by means of Hathaway's apparatus, that the so-called psychogalvanic reflex can be obtained by the use of alternating current.

²¹ The hypothesis of Johnson (18) arose, not from this article, but from acceptance of the doctrine of homeodetic activity set forth in Dunlap's treatment of *Images and Ideas*, published in the *Johns Hopkins University Circular*, 1914, Whole No. 263. The essentials of this doctrine are also implied in Herrick's notion of functional integration, and were expressed still earlier by James.

Because of limitations of space, we are compelled to postpone a general review of studies on learning and forgetting as related to sleep.

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GENETIC STUDIES OF EMOTIONS

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I. METHODS

1. *Individual case studies*

Recent years have witnessed the accumulation of vast numbers of diary observations, clinical records, and psychoanalytic histories. In the present review no attempt has been made to sift this literature for reports on emotions, nor to duplicate surveys covering the topics of mental hygiene, psychopathology or delinquency. Much of this material is of suggestive value, but as a research method the various types of personality studies are not as a rule sufficiently quantitative to provide more than a general background for data collected through other procedures.

2. *Questionnaires*

Two sorts of questionnaires have been used in connection with problems of emotional development: (a) with questions concerning observed behavior in others, (b) with questions concerning the subject's own present or past experience of emotion. The first of these is illustrated by Hall's classical studies of laughter (49) (1897), and of anger (50), which contained such questions as the following: "Recall a few cases of great laughter in children and describe its cause." "Describe overt acts (involving anger): describe every vasomotor symptom, . . . all changes in muscle tension . . . In description be photographically objective, exact, minute and copious in detail." The second type of questionnaire is illustrated by Hall's study of fear (48), in which he circularized 1,700 teachers and school children, requesting information concerning fear tendencies and remembered fear episodes. Evidence obtained in this manner suffers of course from sampling errors and from intrinsic difficulties in following instructions.

The earliest systematic questionnaire covering emotional traits, with especial reference to emotional stability, is the Personal Data sheet devised by Woodworth. With adequate norms and with standardized procedures, a questionnaire of this type becomes a "test." B. Johnson (55) employed an adaptation of the P.D. sheet,

in a study of malnourished as compared with normal children. Mathews (87) constructed a special form of the questionnaire for use with school children; 70 items were included, dealing with fears, worries, unsocial moods, etc.; the reliability was reported as .67, but in a tentative validation through the comparison of problem and normal children, the test failed to demarcate the two groups. E. G. and C. W. Flemming (39) have checked the Mathews Revision against teachers' estimates of emotional balance, finding small negative correlations which were unaffected by partialing out age or intelligence.

A questionnaire in the form of a standardized oral interview, is illustrated by the study of Town (130), who described a series of 80 imaginary situations to the subject, with the request that he indicate "how he would feel or what he would do." The (imagined) situations are considered to be provocative of the fundamental emotions listed by McDougall (fear, disgust, curiosity, anger, elation, subjection, tenderness, sociability). Goodenough (45) has reported an interest questionnaire, with particular reference to the question, "Suppose that a fairy were to grant you three wishes. What would your wishes be?" The results were regarded as of practical value in clinical procedure, for subjects having an M.A. of 9 or over.

The expounders of questionnaires have not been idle in Europe; in fact the method has emerged in places as a respectable branch of "experimental reflexology." This is the case, for example, in the work of Shevaleva and Ergolska (116), which involved a no more rigorous technique than the classification of children in rotating observation groups as "relatively non-excitabile, consistently excitabile, or unstably excitabile." Studencki (126) has employed a questionnaire in studying "children's relations to themselves," and Baumgarten (7) has utilized the unsettled social conditions following the German invasion of Poland, to obtain questionnaire data from children concerning emotional attitudes associated with "hate." Hatreds arising within family groups have been investigated by Robin (109), in a study of French families.

While questionnaire methods have been appraised as belonging to the "underworld of science," they still appear to retain some usefulness in opening new areas for investigation. From standpoints which emphasize the un verbalized character of emotion (Watson, 134), questionnaires would be expected to achieve a lower validity in the field of emotion than in other branches of psychological research.

3. *Trait ratings of children*

One of the earliest uses of ratings in a genetic study was that of Pearson (96) (1904). Ratings of siblings by teachers or members of families were obtained for such traits as "vivacity," "assertiveness," "temper," etc. Data were collected on about 2,000 pairs, but the ratings were roughly made on only a two or three-point basis. Heymans and Wiersma (52) obtained ratings from 3,000 physicians on parents and children, with regard to emotional and other traits; this material was later used in a study of family resemblance by Schuster and Elderton (110), but has frequently been criticised on the ground of non-comparability and the probability of excessive chance errors.

A more careful technique was employed by Terman and Goodenough (128) (1925) in their study of trait ratings of gifted children. A graphic 7-point scale was used, with concrete definitions of each step on the scale, and with the midpoint of the scale serving as "average for age." However, in the case of emotional traits the correlation of parents' with teachers' judgments was so low as to be negligible. In their discussion of mood type, mood stability, and emotional response type and emotional stability, S. and M. G. Blanton (14) have described a somewhat different form of rating scale, based on departures toward either extreme from an "ideal average." The incorporation of ratings into a "mood chart," with data on the influence of emotional disturbance upon later mood fluctuations, has been reported by M. C. Jones (66).

L. Marston (86) has devised an Introversion-Extroversion rating scale, consisting of 20 alternative items, with a provision for a graded estimation of each pair of items; the form of the scale results in a bimodal distribution of ratings. By the split half method, reliabilities of from .83 to .98 were found, while different raters agreed to the extent of an average r of .71. Powers (100) has used this scale with adolescents, reporting results very similar to those obtained by Marston with preschool children.

In a rating chart using teachers' own wordings about behavior, Bridges (20) arranged fifty items in the form of paired opposites, including such emotional traits as "venturesome or timid," "not excitable or excitable," "spontaneous or restrained." On a small nursery school group, ratings were found to correlate nearly perfectly with order of merit ranking of the same children.

Moore has devised an infant rating scale, which was applied by

Bonham and Sargent (18) to 120 children, rated, within two weeks of birth, by nurses in a maternity ward. Thirty-five of these were followed up by ratings on the Bonham-Sargent scale at 24 and 30 months. A number of emotional traits were considered, and correlations studied for traits at successive ages.

In a review of rating methods, the writer has commented, (58) "It is doubtful if ratings, taken by themselves, can contribute anything of fundamental importance in the study of child development. Their value lies in yielding certain background information, of a greater degree of compactness and (apparent) definiteness than the data in diary records." Granting that the validation of other procedures rests eventually upon some form of rating, it nevertheless appears to be true in psychology as in other sciences that our fundamental gains lie in the direction of more objective methods in measurement.

As Goodenough and Leahy (47) have shown, ratings may have a definite value in the differentiation of groups. They obtained significant results in a comparison of birth orders on the basis of teachers' ratings. The legitimacy of this method rests, of course, upon the uniformity of the procedures used by different raters, the proper distribution of the groups to be compared, and in certain cases, upon keeping the raters in ignorance of the particular problem which is being studied.

4. *Inventories of child behavior*

The difference between questionnaires, ratings and inventories is often merely nominal. A rating may be regarded as a more quantitative type of questionnaire, while an inventory attempts a systematic survey of objectively noted characteristics.

The Andrus inventory (5) contains many items similar to those appearing in the Woodworth P. D. sheet, but is checked and scored on the basis of observers' records of behavior. In the hands of a trained person, the instrument is claimed to be "probably as objective as the usual mental test." Norms, however, are not regarded as feasible, and the value of the inventory is stated as ancillary to ratings, and to an understanding of a child's relative development in the several groups of functions represented in the inventory.

Yepsen's (141) personal-behavior score-card is another example of an inventory, designed to appraise maturity of development with reference to a large number of emotional and other traits.

5. *The method of repeated short samples*

Olson (95) and Goodenough (46) have applied a method involving twenty or more behavior observations, covering a period of from one to five minutes. The scoring for each period is based on the presence or absence of certain specific and objectively defined forms of behavior. Olson obtained reliability coefficients for single age groups of from .4 to over .8, in the measurement of such nervous habits as thumb-sucking, nail-biting, etc. Goodenough found that on such a trait as "anger," two observers (not working simultaneously) would obtain scores correlating to the extent of .7. An increased number of periods was found to increase reliability in agreement with the Spearman-Brown prediction—a significant finding from the standpoint of controlled measurement.

6. *Paper-and-pencil tests*

Elonen and Woodrow (33) have employed a free association test as an indication of psychopathic tendencies in children. With sixth grade children a reliability of .86 was obtained, and a correlation of .57 between pathological association scores and composite ratings of behavior. An oral free association technique would no doubt be applicable in lower age ranges; in children with limited vocabularies it remains to be determined whether an idiosyncratic association would be more or less significant than in older children.

Furfey (41), attempting a measure of an assumed variable which he terms developmental age, has devised a test with norms down to the age of eight years. Chambers (29), working with the Pressey X-O test of the emotions (101), has established a differential score which correlates $-.12$ with intelligence, but shows an increase in medians from grade to grade. Tjäden (129), using the Pressy X-O with a group of delinquents of high I.Q., found that it failed to discriminate them emotionally from Pressey's original sample of college students. It was stated, however, that the tests were valuable "in revealing constellations of ideas which have marked emotional content." Tests of this nature, including also the Colgate Mental Hygiene inventories and the Downey Will-Temperament profile, are of course chiefly adaptable for use with adolescents and with adults. Raubenheimer (103) has reported considerable success in the use of a series of discriminatory tests with young delinquents. In this, however, we are approaching the field of character tests, which has been elsewhere very adequately reviewed.

7. Tests of emotional responses in laboratory situations.

The testing of pain sensitivity by the earlier child biographers and physiologists, was conducted chiefly from the standpoint of problems in sensory psychology, rather than with reference to problems in the emotions. The schedule of provocative situations used by Watson and Morgan (137) (1917) in their study of love, anger and fear represented an important departure in the direction of a laboratory study of the emotions. A later description of these situations, with photographs, may be found in Watson (136).

L. Marston (86) (1925) devised a series of experimental situations for the study of social resistance, compliance, caution, degree of interest, and self-assertion. The child's behavior in a standardized situation is scored with reference to 5 or 6 defined degrees of "extroversion." Intercorrelations of the various tests ranged from .04 to .61. The average correlation with introversion-extroversion ratings was of the order of .4. Marston gives a useful bibliography of 127 titles.

H. E. and M. C. Jones (60) have reported a series of laboratory tests of the response to animals, conducted over an age range of from fourteen months to twenty years.

C. Buhler (22), working with over 100 children up to 22 months of age, studied reactions of domination and submission by placing two children together with a single toy.

Meyer (89) conducted an experiment with 30 nursery school children, involving proximity to a guinea pig which was at large in a laboratory room. Ratings of eight different phases of reaction were made at several points during the experiment.

Reynolds (105) has attempted to test negativism in preschool children through a series of standardized situations, the most important of which were called "surrender" and "imitation." Fairly reliable scores of negativism were reported, which were found to be independent of I.Q. and educational background, although inversely related to C.A. and M.A. The decrease of negativism, in children from twelve to forty-eight months, has also been studied by Zaluzhni (142).

May's (88) studies of children's responses to a variety of disturbing or distracting stimuli may also be classified here, although his test batteries were primarily concerned with measuring the inhibition of overt movement.

Laboratory studies give considerable promise of adding to

our knowledge of emotional development, particularly when they are conducted cumulatively on the same children, and with due regard to the child's general reactions. Up to the present time, these experiments have lacked a unified program, and have suffered from a too desultory and too sporadic method in the approach to basic problems.

8. *Instrumental procedures*

Instrumental studies of children's emotions have developed very slowly, perhaps chiefly because of the modification required in the usual apparatus techniques before they can be applied in the lower ages. Volkelt (132) (1926) has reported a summary of fifteen years of European research on children. The only experimental report which appears to bear upon children's emotions, is that of Canestrini (27), who obtained kymograph curves of respiration and fontanelle pulse in the case of 70 infants, ages six to fourteen days. Unpleasant tastes produced evidence of emotional disturbance, while a milk and sugar solution was stated to have a quieting effect. It was also claimed that the curves showed a marked flattening when the mother whispered to the child, while strange voices had no effect.

A pioneer instrumental study was that of Binet and Courtier (9) (1897). In plethysmographic observations of a small number of children, eight to ten years of age, characteristic changes were noted in response to emotional stimuli (a reduction in pulse height and a fall in the volume curve).

Eng (35) has made a more extended test of this method, using a pneumograph and a plethysmograph in a comparative study of eleven children (ten to twelve years of age) and fourteen adults. Excitement was characterized by a high pulse and a high volume curve, with occasional irregularities in the respiration curve. She believed that the kymograph record revealed characteristic differences between unpleasant and pleasant emotional states, and that these could be further classified according to their origin in sensory stimuli or in "spontaneous" processes.

H. E. Jones (57) has developed a technique for galvanometric measurement in infancy, employing silver foil electrodes which are covered with kaolin paste and bandaged to the sole and calf of the leg. The psychogalvanic reactions obtained to a variety of provocative stimuli show the usual characteristics of the responses found in adults (long latent period, a typical curve form, and a quick

exhaustion of the response by repeated stimuli). The value of the instrumental registry lies in the objectivity of the records, and in the possibility of working with less intense stimuli than those employed for ordinary observational studies. His results are in opposition to the earlier negative findings of Peiper (98), in galvanometric studies of children under one year of age.

In later reports of work with nursery school children, Jones (58, 59) has described a galvanometric apparatus involving a continuous roll kymograph equipped with accessory ink pens for the registry of stimuli and of specified phases of overt behavior. Individual differences were recorded in initial resistance, resistance trends, resistance rhythms, and in the magnitude of psychogalvanic responses to a standard schedule of stimuli.

Denisova and Figurin (31) have studied the respiratory variations in infants by means of the Lehman pneumograph. While they were primarily concerned with rhythmic changes during sleep, their data on the relationship of respiratory cycles, urination, and overt motor phenomena are of great interest from the standpoint of a study of physiological factors underlying emotion and mood.

A method for registering somatic effects (or accompaniments) of emotion has been described by B. Johnson (56). Pressure applied to a stylus is recorded on a drum, and variations in muscle tension can be read with reference to a "tension base." The method is applicable with children as young as two years.

9. *The method of "co-twin control"*

The rôle of hereditary factors in emotional development has been studied by Gesell (43) from cumulative records of the emotional behavior of identical twins. Personality and temperament data on identical twins reared apart have been recorded by Müller (93) and Newman (94).

II. THE "PRIMARY EMOTIONAL PATTERNS"

The child biographers who described emotional processes in infancy were not as a rule concerned with systematic formulations; the listing of the "primary emotions" was left to more speculative writers.

In 1917 Watson and Morgan (137) contributed a list of primary emotions based upon empirical observations of infants. These were (1) fear, in response to loss of support or loud sounds, (2) anger, in response to hampering of movements, and (3) love, in response

to stroking or manipulation of some erogenous zone, or tickling, patting or gentle rocking. The shock effect of sounds had been previously discussed by Shinn, who agreed with other observers as to the great variability of the emotional reaction to auditory stimuli. Shinn (117, p. 209) has also noted the efficacy of equilibrial disturbance in producing fear, citing notes from Tiedemann, Mrs. Hall and Mrs. Moore. Watson omits mention of the emotional reaction to pain. Although in infants the pain threshold is high, there can be no question as to the ability of pain stimuli to elicit emotional responses even during the first few weeks. Sherman (112, 114), H. E. Jones (57), and Tayler-Jones (127), report infants' reactions to cutaneous pain stimuli, administered under laboratory conditions. Earlier observers (as Kroner [76], and Preyer [102]), have also made records on this point. An excellent review of the earlier literature is given by Shinn (117, pp. 32-37).

The definition of the sources of anger and fear exclusively in terms of stimuli has been challenged by a number of writers. In a study of responses to a snake, H. E. and M. C. Jones (60) have shown an absence of fear behavior under two years; from two to five, fear of snakes is positively correlated with age, even when the possibility of specific conditioning is ruled out. This has led to a definition of the native fear conditions in terms emphasizing the "preparedness" of the organism, rather than purely in terms of the objective characteristics of the stimulus. A similar view is advanced by English (36), and is of course not unfamiliar in the theoretical literature on the emotions.

Blanton's (13) study of crying during the first month of life is one of the best analyses of the stimuli to crying, and of the response patterns. Crying was elicited by a variety of noxious stimuli, by hunger, and probably also by fatigue and deprivation from exercise. Crying in the newborn, on the basis of observations of 98 infants, has been studied by Bryan (21). Watson (134) has reported on a study made by M. C. Jones concerning the common sources of crying and laughing in institutional children (sixteen months to three years). Eighty-five situations were listed, the most common sources of crying being "having to sit on the toilet, having property taken away, being left alone, working at something which won't pan out." The most common sources of laughing were "being played with, romping with other children, playing with toys, teasing other children, watching other children play."

Lippmann (82) has reported crying as showing its greatest frequency between six and one-half and ten months.

Enders (34) has also reported an observational study of laughing, with evidence for the provocative nature of sound, motion, and social situations.

The development of smiling, in response to a visually mediated social stimulus, has been described by M. C. Jones (65) in a sample of 185 cases. The youngest child to smile in a prescribed situation was thirty-six days of age; the response appeared in 100 per cent of the children of ninety days or older, with a somewhat earlier maturation in negroes than in whites.

Watson (136) has reported several incidental studies of jealousy. Berne (8) has found three-year-olds more rivalrous and more jealous than two-year-olds. Foster (40), in an investigation of 50 habit clinic children one to six years of age, compared with a control of 100 "non-jealous" children, found that the jealous child is more often a girl between three and four, and frequently the oldest child; she is subject to mild neurotic fears, and shows a tendency toward markedly extroverted behavior. The employment of control groups in certain types of clinical studies is an important step in the direction of a more adequate research use of this method.

In a study of the major emotions of mental defectives, Morrison (91) obtained data on over 200 institutional cases, 35 of whom were under ten years of age. Anger and affection were found to be frequent in all grades except the lowest idiots, and ratings on each of these correlated approximately .7 with intelligence. No relation was found between fear and intelligence.

The response patterns involved in the emotions of rage, fear and love have been described by Watson and Morgan (137), and in several later texts or semi-popular presentations by Watson (133, 134, 136). Fear involves a checking of breathing, a bodily start, crying and visceral responses. Anger involves a temporary cessation of breathing, stiffening of the body, screaming, reddening and later "bluing" of the face, and visceral responses. Love responses are indicated as involving a cessation of crying, with gurgling, cooing and visceral reactions. In a series of ingenious experiments, Sherman (112) has demonstrated the inability of observers to differentiate the emotions of infants, unless aided by knowledge of the stimulating situation. If emotions cannot be defined purely in terms of stimuli or of responses, we are forced to resort either to the con-

cept of an undifferentiated emotion of "excitement," as advanced by Stratton (123), or to a classification of the emotions based on functional relationships.

III. THE MODIFICATION OF THE EMOTIONS

The possibility of conditioning the emotions was demonstrated by Watson and Rayner (138) (1920) who conditioned an eleven months child to the sight of a white rat, through four associations with a loud clang. Evidence was given for a diffusion of the response to other animals and to various furry or hairy objects. A month after the last conditioning, the directly conditioned and the transferred emotional responses were still present to some extent.

Moss (92) conditioned two children, two and four years of age, to the sound of a snapper by the use of (a) the taste of vinegar and (b) cutaneous pain stimulation. In the first case a spreading of the conditioning was noted, to other phases of the situation.

Skerrett (118) has reported the conditioning of a seven-months infant to an olfactory stimulus. Evidence for the conditioning of visceral processes was given in a much earlier study by Bogen (17) (1907) who reported the establishment of a C-R between a sound and the flow of gastric juice, in a child of three years.

Conditioned psychogalvanic responses were first reported by H. E. Jones (57) (1927) who worked with infants from two to five months of age, associating previously indifferent visual and auditory stimuli with a mild electrical stimulus applied to the skin. The phenomena of extinctive inhibition were demonstrated (as distinct from ordinary P.G.R. exhaustion), with re-appearance of the C-R after an interval. C-Rs established by from 4 to 14 conditionings, were shown to survive at least three weeks without any form of reinforcement.

English (36) has described two instances in which a fear was transferred to an associated stimulus previously incapable of eliciting fear. In a third instance, an attempt was made to condition a child to a loud sound, but was unsuccessful because of the ineffectiveness of the primary stimulus. English believes it is not yet established that these transferred emotions can be classified as examples of "conditioning."

Blatz and Bott (16) have given illustrations of conditioning by shock and conditioning by social contagion, as contrasting with the usual laboratory method of conditioning by repeated association.

Anderson (4) has described an interesting case of the reappearance of a lapsed fear, which he attributes to reconditioning through a dream.

Watson depicts the complex emotional life of the adult as the outcome of numerous conditionings of three elementary patterns. Allport (3) has formulated these changes in greater detail, in terms of efferent and afferent modifications. On the basis of laboratory studies, however, Gesell (43) and H. E. and M. C. Jones (60) have emphasized the rôle of intrinsic maturation in the development of emotional patterns. Evidence is cited for the late appearance of specific fears which could not readily be explained in terms of conditioning.

The elimination of fears by laboratory methods has been studied by M. C. Jones (61) (1924), who reported on results from the following methods: Elimination through disuse, verbal control, negative adaptation, repression, distraction, direct conditioning, and social imitation. The latter two methods were the only ones meeting with unqualified success. Direct conditioning involved associating a fear object with a "craving-object," and replacing the fear by a positive response. A more detailed report on this method was given by the same writer in (62), with two subsequent summarizations (63, 64).

IV. PERIODICITY IN THE DEVELOPMENT OF EMOTIONS

Levy and Tulchin (80) (81), in a study of baby conference children, six to fifty-four months of age, report that in test situations the most intense and the most frequent resistant actions centered about the 18th month for girls and the 30th month for boys. Keens and Blatz (69), in a study of nursery school children, have found a greater frequency of emotional episodes in three-year-olds.

Busemann (25) has reviewed the literature on periodicity, and on the basis of a considerable number of studies has concluded that certain critical phases or periods of excitation occur at ages three, six, nine, twelve or thirteen, and sixteen or seventeen. The "emotionality crises" at three and six are evidenced chiefly by data from language studies.

In a study of misdemeanors in school, Blatz and Bott (15) have reported a striking increase of misdemeanors in boys of the eight and nine-year old groups, with a smaller peak at thirteen-fourteen.

Both Hetzer (51) and Ch. Bühler (23) have reported a pre-pubertal "negative" phase.

In Hall's (48) study of fears, periodicity is either absent or obscured by the characteristics of the data collection; boys' fears were said to increase from seven to fifteen years and then to decline, girls showed an increasing frequency of fears from four to eighteen.

Lehman and Witty (77, 78), in their study of play interests, have found no evidence for periodicity, and have concluded in favor of a gradual growth rather than of sudden jumps in development.

In an investigation of the emotional attitudes of 230 clinic children, through a statistical study of dreams, Blanchard (10) has failed to find the influence of any specific age factor, except that relatively few dreams are reported by children under six.

Regensberg (104) has discussed emotional maladjustment in gifted children, finding some evidence of periodicity which is environmentally determined by accession into school groups of equal intellectual level but of superior emotional maturity.

Cyclical variations in mood in infancy have been discussed by Shinn (117, pp. 213-214), without any attempt at quantitative expression.

The study of short-time as well as of long-time rhythms in emotional pattern appears a promising field for coöperative study by psychologists and physiologists.

V. EMOTIONAL "TYPES"

Rich (106) has examined blood specimens of 303 children for inorganic phosphorus and creatinine, and obtained correlations with ratings for such traits as good nature, perseverance, leadership, etc. Rich has concluded that chemical mechanisms contribute to the total personality make-up to an extent represented by correlations of from .2 to .3. It is clear that the biochemical approach cannot henceforth be overlooked in studies of emotional characteristics.

Emotional correlates of the "T-type" and the "B-type," in relation to calcium metabolism, imagery characteristics, etc., have been discussed by W. Jaensch (53). Jaensch's type theories are reviewed by Gesell (42). Hallucinatory experiences as an expression of emotional difficulties, have been discussed by Sherman and Beverly (113).

Peters (99) has made an analysis of school failures, classifying

them in three types. An undetermined type (similar to Meumann's verbal and Binet's unstable type) shows a poorly organized and purposeless excess of activity; a passive or inactive type develops slowly because of a deficiency in experience, and is similar to Binet's "arrested type." A third type comprise the "feebly inhibited."

Kretschmer's (74) type theories, which have been vigorously criticised by Kritsch (75) as a throw-back to an earlier physiognomical point of view, have received some support from Krausky (73) in a study of 100 school children; the latter claimed that four out of five conformed to the constitutional type classification. The behavior relationships to these types are not yet satisfactorily interpretable in genetic terms.

In a study of delinquents nine to eighteen years of age, Wires (140) found no profile characteristic of any type of delinquency; the composite profile of the total group showed high scores for impulsion and low scores for resistance and inhibition. The general literature on type theories has been reported by Klüver (70, 71).

VI. RELATED VARIABLES IN EMOTIONAL DEVELOPMENT

The influence of Watson in laboratory studies, and of a clinical or therapeutic point of view in field work, have led to an emphasis of environmental determinants of the emotions.

The earlier studies of hereditary factors have in recent years been limited chiefly to problems involving intelligence rather than temperamental characteristics. Pearson (96), however, has given evidence for a degree of familial resemblance in emotional traits approximating that found in physical or intellectual traits, and has interpreted this in favor of the influence of heredity.

Davenport (30) and Finlayson (38) have perhaps over-simplified the problem, by describing the inheritance of excitability in terms of a Mendelian dominant character. Levy and Patrick (79) have considered familial factors manifested in the production of children's temper tantrums and other emotional manifestations. The numerous genealogical studies which have been made on the subject of neuropathic inheritance will not be summarized here. Reference has already been made to studies of the emotional characteristics of identical twins reared apart (93, 94). The evidence is inconclusive.

Birth order as a factor in emotional development has been studied by a number of investigators. Stratton (122), employing self-ratings of the degree of fear and anger experienced in a large number of concretely described situations, reported the first-born as more subject to anger. Goodenough and Leahy (47) used teachers' ratings of 293 kindergarten children. The first-born obtained significantly lower ratings in aggressiveness and self-confidence, and higher ratings in suggestibility, seclusiveness and introversion. Stuart (125) studied deviations from normal emotional expression in 465 young men tested by the Colgate Mental Hygiene inventories; deviations were unrelated to birth position, but showed a slight increase with size of family. Busemann (26), in a study of school children, has reported an inverse relation between size of family and the occurrence of hyperactive or introverted tendencies, while Slawson (119) has given evidence for a slight but significant correlation between size of family and male juvenile delinquency. Breckenridge and Abbot (19) and Pearson (97) have found a greater incidence of delinquency or criminality in the first born.

Fenton (37) has briefly reviewed the literature concerning undesirable social traits in the only child, citing quotations from Bohannon, Hall, Mead and Abel, Wexburg, Brill, Coriat and others. The common belief that only children are deficient in emotional stability, was tested by a study of teachers' ratings of approximately 200 children in an elementary school. No significant differences were found between only and other children. Similar negative results have been reported by Stratton (122) and Stuart (125), while Blatz and Bott (15) find that only children have the fewest misdemeanors reported in school. Goodenough and Leahy (47) have obtained for only children higher ratings in aggressiveness, instability of mood, and flightiness of attention, and Burt (24) in a delinquent group has found a higher proportion of socially "only" children than in a control non-delinquent group.

The inconsistent results obtained in these investigations are no doubt to some extent due to sampling errors involved in the use of "marked cases." In further more analytic studies of the influence of birth rank, cases should be selected with reference to such factors as the relationship of family size to intellectual and social status; the relationship of pregnancy order to pre-natal and parturitive conditions, and the phenomena of social interaction in family groups of varying sizes.

The influence of birth trauma upon the development of emotional behavior has been discussed by a number of writers, of whom the most recent is Schroeder (111).

The effect of organic disturbances such as encephalitis has also received considerable attention. Koseki (72), to cite one example, has given a number of case histories of children who suffered a marked change in temperament after a cure of encephalitis lethargica: an increased assertiveness and excitability, with no accompanying changes in intellectual status.

Stratton (121) (124) has studied the relation between emotion and the incidence of disease; persons who have been subject to disease tend to respond more intensely to anger situations and probably also to fear situations. A history of disease before the age of 6 appears particularly significant in relation to later tendencies to anger. Stratton conservatively points out that the relationship may rest upon (a) a common constitutional predisposition, (b) the effect of disease in predisposing emotion, (c) the effect of emotion in predisposing disease.

Data on intellectual development as influenced by an endocrine disorder (puberty praecox) have been surveyed comprehensively by Doe-Kulmann and Stone (32). In view of the relationship between the emotions and the endocrine organs (reviewed, up to 1925, by Rikimaru (108), it would seem well worth while to make a thorough genetic study of emotional correlates in endocrine disorders.

Lippmann (83), on the basis of results from the administration of atropine, has emphasized the importance of congenital autonomic unbalance, in the production of certain types of emotional disturbance in infants.

Environmental factors influencing the emotions cannot be reviewed at all fully without covering the whole province of mental hygiene. Weill (139) has presented a review of recent discussions on this problem, together with data from 17 families purporting to show the dominant influence of environment. Blanchard and Paynter (12) have investigated the emotional reactions and family adjustments in a group of 80 children from economically "marginal" families. No evidence was found for an economic handicap affecting normal emotional adjustment. Blanchard (11) has elsewhere listed, in very general terms, four "outstanding factors which contribute to failure in socialization": lack of discipline in home, a broken home, lack of recreation, influence of bad companions. Gesell and

Lord (44), in a comparison of small groups of nursery school children from two widely separate economic strata, found at this pre-school age a reduced spontaneity and greater expressional inhibition in children from homes of inferior status.

VII. THE FUNCTIONAL SIGNIFICANCE OF THE EMOTIONS

Under this topic no attempt will be made to review more than a few recent illustrative references.

The expression of emotional disturbance in various forms of conduct disorder is of course a major topic of inquiry in the field of psychopathology. Evidence obtained by working backward from symptoms to assumed causes is commonly subject to error through lack of conclusive verification. This has often been pointed out in criticisms of psychoanalysis, but is no less true in connection with other types of clinical inquiry, unless particular care is taken in the matter of controls.

A representative example of inference from clinical observation, may be found in the recent report by Lowry (84), who discusses the effects of inferiority (in intellectual, social or physical traits); these effects may be manifested in withdrawal, compensation by developing excellence in some other trait, or over-compensatory expression in such a form as pathological stealing or lying.

Lying as a function of conflict involved in various emotional situations has been studied by A. M. Carmichael (28). The material used was a collection of 1,200 descriptions of the reactions of six-year-olds. Instead of lying, frequent alternative responses were crying, or acts of submission or combativeness. The fear motive as contributive to lying has also been emphasized by Baumgarten (6). Riddle (107) has reported on stealing as a form of aggressive behavior.

In a study of 7,664 clinic cases, Starr (120) has found among pre-adolescents only a very small percentage with a record of emotional disturbance. With adolescent delinquents, however, emotional maladjustments were shown to be of major importance.

The influence of emotion upon the nutritive processes in children has been made the subject of reports by Mohr (90), Aldrich (2) and others. J. L. Kantor (68), in a discussion of neurogenic and psychogenic alimentary disorders, gives a comprehensive bibliography of 77 titles, dealing with this subject, with titles up to 1928.

Emotional factors in the causation of enuresis have been frequently discussed in the clinical literature. A recent example of a statistical study is that of Ackerson and Highlander (1), who reported a low positive correlation between enuresis and such personality problems as irritability and seclusiveness, in a clinic sample of 3,000 cases.

The influence of emotion upon mental test performance has been studied by Jewett and Blanchard (54) and MacKaye (85). In the case of infants, Levy and Tulchin (80) have reported resistant behavior in over one-third of the subjects during mental test situations.

The relation of emotion to disorders of the language functions involves a literature so large as to require separate treatment. The most recent bibliography on speech pathology is that made by L. E. Travis (131) in 1929.

Both the behaviorists and the psychoanalysts have emphasized the importance of the emotional experiences of early childhood. Watson (136) has stated, "At the age of three the child's whole emotional life plan has been laid down, his emotional disposition set." On the other hand, Judd (67) and others have commented on what they consider the current over-emphasis of childhood experiences. In this, as in so many instances in the discussion of emotions, the literature is more richly supplied with speculative fancies than with actual data. Undoubtedly the chief present need is a scheme of research which will organize our methods and personnel towards a more systematic attack upon genetic problems.

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SPECIAL REVIEWS

DE LAGUNA, GRACE ANDRUS. *Speech: Its Function and Development*. New Haven: Yale University Press, 1927. Pp xii+363.

The author divides her book into three parts. Part I deals with the social function of speech; Part II gives "a preliminary account of the general psychological scheme" which is the basis of Part III; the latter discusses the function of speech in the life of the individual. We may conveniently treat the book as attempting two tasks: first, that of presenting a thesis concerning the function and development of speech; and secondly, that of developing certain psychological principles. Let us consider the latter first.

In her preface, the author, in acknowledging her indebtedness to the philosophical writings of Professor Edgar A. Singer, Jr., writes, "The general behavioristic position adopted in the present inquiry is, I venture to believe, substantially the same as that which he was the first to formulate and which he has so brilliantly defended." The reader is therefore prepared to find the author's psychological basis dominated by two important features of Professor Singer's psychological theory; namely, the concepts of teleology and of "mind as behavior."

To the reviewer, the concept of "mind as behavior" suggests the figure of a number of shells, under certain of which, he is given to understand, "mind" is to be found; an understanding which he can however never verify; for as the clever operator raises each shell, there is nothing to be seen but behavior. This impression of the concept is strengthened by Professor de Laguna's development of it. She begins by pointing out the futility of that type of "empiricism" (so popular with the writers of current textbooks) which consists in avoiding all explicit statement of theoretical assumptions. She brings to view the metaphysical befuddlement which has led psychologists to call the subject of an experiment an *observer*. She declares for the method of behaviorism and says, moreover, that "it is only by definitely abandoning the assumption of a dualism that our present investigation is made possible." But it turns out that for her, "As a metaphysical theory, behaviorism replaces the dualism of mind and body with a monism, and endeavors to interpret

consciousness in terms of the organism and its behavior. With behaviorism as a metaphysical theory we are not here concerned, except to note its relation to psychological method. We shall not ask whether ultimately there is or is not any distinctive 'mental' existent, or whether the monism which behaviorism advocates is or is not 'materialism.' These questions are fundamental questions, and would merit the most careful consideration were our purpose less special. Our concern is with behaviorism as a theory of psychological method. As such a theory it stands for a complete and thoroughgoing description of mental phenomena in terms of objective conditions, including under these both the organism and the external surroundings in which it lives and acts" (page 127f). Concerning the above I would remark, first, that to apply the term *behaviorism* either to the metaphysical activity of "interpreting consciousness" or to the methodological activity of "describing mental phenomena" would strain that sadly overworked word beyond its limits; and secondly, that however special our purpose, to refuse an answer to the question whether we assume or do not assume ourselves to be dealing, ultimately or otherwise, with a "distinctive mental existent" is hardly to make explicit our principles. Nor do I believe that the scientific psychologist can safely continue to regard his fundamental postulates as belonging to "metaphysics."

The concept of teleology appears in the definition of a response as "not a definite bodily movement regarded as a change in spatial position, but rather a movement, or series or complex of movements, taken in its objective relation to some more or less distinct end which it normally serves." It is "adaptive with respect to specific factors in the environment." Locomotion and the throwing out of pseudopodia, *e.g.*, are not responses because they serve no single end, but occur as parts of many different responses or even occur *aimlessly*. "The fish often swims aimlessly about when he is not actively in pursuit of food or in flight from an enemy." It is not clear that the author recognizes this to be a fact, not of fish but of human psychology. Regarding the fish as a fish, we must recognize that under continually varying vital conditions continually varying responses are occurring, and that in all of these responses the same fundamental principles and mechanisms are involved. "Pursuit," "flight," and all other "ends" are linguistic classifications of responses with respect to their status in *human social interactions*. The author justly declares that "what as psychologists we

are primarily concerned with is not the physiological process as such, but the *identity of functional relationship* between varying processes," and further, "Psychological science, like every other science, must discover and formulate its own principles of individuation." But we ought to become clear that we do not "construe the acts of the organism in their relation to the physical world, and in so doing establish their psychological status"; that "ends" do not exist in nature apart from human interactions; and that the measuring device which we actually use in determining the "psychological status" of a response is the response which it elicits in other human beings. It is by means of this measuring device that we arrive at what Weiss has termed "biosocial equivalences."

Human behavior is described as characteristically made up, not of fixed type-responses, but of "complete acts," which are built up under varying conditions of individual life out of innumerable "functionally independent elements." The mechanism by which this organization proceeds is that of conditioning; the author, however, although describing the process (in a manner similar to that adopted by Smith and Guthrie), does not use the term; instead, she prefers to speak of an "affective transformation" of a stimulus. If we seek to determine what is meant by an "affective transformation," we find it said that "in so far as any stimulus tends to set up activities which normally lead to its own continuance, it is positive, or pleasant"; and further, "we are not to suppose that the pleasantness or unpleasantness is something additional to the behavior in question, a further consequence—or cause—external to the activity in question." But in the discussion of the development of speech in the child, we find repeated references to the "delight" and "enjoyment" of the child in its vocal activities; it is not easy to persuade oneself that no causal efficacy is implicit in these terms. So, too, in the discussion of "Space Perception in the Child" we read, "The sight and touch of the pile of blocks rouse in him vague longings. . . . By accident, at first, he brings two blocks together in a way that pleases him," etc. Such description has an air of "sympathetic insight," but it does not really contribute to the scientific *analysis* of the processes involved. Similar remarks might be made concerning the alleged rôle of the "representative image." This curious changing of the basis of analysis at certain points seems to the reviewer a direct consequence of the author's separation of "metaphysical" and "methodological" postulates.

The author devotes most of Part II to an analysis of the factors whereby behavior, in phylogenetic and ontogenetic development, becomes more and more assimilated, in specificity and complexity, to the specific objective features of the environment. The lower animals have a few type-responses, adapted to a few categories of environmental situations; man has a large number of specific responses to specific features, including such "properties" as length and shape. The author shows the importance of the social factor and of the use of tools in developing responses to objects *in their relations to other objects*, rather than in their relations merely to oneself. But throughout this discussion the author seems to envisage three correlative lines of development: as *behavior* becomes more specific, so too does *perception*, so that there is an increasing development and "objectification" of the "*psychological environment*." The methodological advantage of repeating the analysis of the first of these in the names of the other two is not clear. It is only in a figurative, and hence in a scientifically bad sense that the psychologist can be said to study the development of the environment.

In Part I, the author attempts to trace the development of speech from the animal cry. It is assumed that the primary function of both cry and speech is that of coördinating the activities of the members of the group, and that the evolution of speech from the cry was guided by the necessity for a more flexible type of group organization under changing conditions of life, as in the assumed change from arboreal life to ground-dwelling. This notion of the basic function of speech is contrasted with the traditional view that language is fundamentally the means of expressing or communicating "ideas"; Wundt's doctrine of "expressive movements" is taken as an example of the sterility of this view which proceeds from the pre-conceptions of a metaphysical dualism. The work of Whitman and Craig on the behavior of pigeons is taken as a basis for an analysis of animal cries. The distinguishing feature of animal cries is said to be their direct connection with specific type-responses. "Instead of saying that they express feelings or emotions, we may say that they occur as elements of such responses." Similarly, the cry as a *stimulus* evokes chiefly type-responses in other individuals. Speech, on the other hand, does not occur as a mere element in a larger response, nor does it release one of a few large and relatively uniform modes of response. "The correlation between the speech-

response and its objective conditions is a correlation between independently variable elements of response and independently variable elements of the external situation, or of past or future events." The evolution of the basic *structure* of language from the animal cry has been conditioned by the increasing complexity of coördination in group life. The author believes that the evidence afforded by gesture-language, hybrid jargons, and the speech of the child (especially the "spontaneous" child languages) points to "a certain priority of a structure analogous to the isolating type." Explicit predication arose "from the combining of formerly independent terms." These formerly independent terms were *sentence-words*, in which predication was only implicit. The differentiation of words as units of analysis resulted from the extension of terms from one situation to others having objectively common properties. A typical way in which predication may have become explicit would consist in substituting for the bodily orientation or pointing which accompanied an implicitly predicative proclamation another utterance which designated the general situation. This evolution of the developed sentence structure was brought about by (1) the need for coördinated action beyond the limits of the common perceptually present situation, and (2) the need for coördinating such complex and varied behavior as depends on analysis of the situation.

The merit of this presentation lies in the author's clear exposition of the fact that vocal responses are in their biological origins of the same nature as all other components of pattern-responses; they are indicators, not of "feelings," or "emotions," or of "ideas" crying for expression, but of the nature of the total responses of which they are merely components. Being auditory stimuli, they readily become substitute stimuli for other individuals. In putting this matter clearly, the author has performed a real service. But her analysis of the subsequent development of true language, with its differentiation of structure, is of more doubtful value, and here again it is the intrusion of the concept of teleology which seems to interfere with a useful analysis. At every point the author seeks to show that the assumed developments fulfilled certain "needs"; that the requirements of social control "demanded" this or that development; whereas from the scientific point of view we are much more interested in isolating as many as possible of the *antecedent* conditions which made inevitable the developments in question. These antecedents are on the one hand the biological properties of human beings, particu-

larly the modes in which their behavior undergoes variation, and on the other the environmental, particularly the social, stimulating conditions to which they are subjected. However useful we may demonstrate language or any particular development in linguistic structure to have proved itself, we do not thus arrive at those laws of behavior which will alone constitute the scientific solution of the problem of the origin and development of language. It is not even clear how such an exposition will help us in the formulation of research problems.

Of chief interest in Part III is the analysis of the developments in individual behavior which result from the elaboration of *conversation*, which is regarded as "primarily the preparation for concerted or socially determined action." Two phases or stages in conversation are distinguished. "The first is the determination, through analytic description, of *what* the situation is in which action is to take place. The second is the formulation of a plan of action." The internalization of the first phase is regarded as the basis for belief and thought; the internalization of the second, as the basis for "conscious purpose." Free memory and imagination are held to result from the freedom and independence of (internal) speech, the course of which in these cases is usually directed by "desire." It is not clear whether the author means by "desire" a psychical force, interoceptive excitation, or verbal self-stimulation.

The "autonomous" nature of language is excellently presented. In current discussions of language by psychologists chief emphasis is usually placed on the *substitutive* nature of speech responses, and the fact is not always sufficiently recognized that language has become an activity in and for itself, consisting of responses far more complex, numerous, and specific than the repertoire of manipulating responses, and organized in structural patterns peculiar to the various languages.

In its emphasis on objective, genetic, and social factors, this book gives a good idea of the progress that the psychology of language has made since the publication of Wundt's *Völkerpsychologie*.

University of Washington

ERWIN A. ESPER

PURDOM, T. LUTHER. *The Value of Homogeneous Groupings*. University of Research Monographs, 1928, No. 1. Pp. 96.

The value of homogeneous grouping on the basis of intelligence was investigated in six high school groups. The first year pupils

were estimated to be approximately equal as to age, sex, intelligence score and the fact that they were taught by the same teacher. Each school provided a control group and experimental group divided into two to five sections, depending upon the size of the school.

The intelligence tests and the English and Algebra Achievement Test Scales were chosen on the very doubtful basis of volume sale. The teachers were instructed to teach "all she could of her subject with entire freedom of judgment as to method."

The author lists some eight advantages ordinarily accredited to homogeneous grouping: relating to variation and method of teaching different levels of intelligence, encouragement and progress of pupils, ease of instruction and enrichment of curriculum. Complete tables of distribution are given for each school with a summary of results of five schools studied. The gain in the standardized tests in terms of mean and median scores proves no advantage in favor of the experimental group; however, both English and Algebra semester grades were in their favor.

Teachers were asked to express their opinions on the basis of eleven questions. The opinions were greatly divided and did not correlate with the results attained in their sections. This was especially true in the case of the progress of dull pupils: where teachers felt they had gained most, the control sections maintained better standing.

He concludes that pupils in homogeneous sections neither gain more when results are measured by standardized tests, cover more course material, nor put forth greater effort. These results hold true for pupils of varying degrees of intelligence measured by standardized tests.

The study ignores almost entirely the analysis of the student's reaction or his motivated attention. The value of such grouping as an incentive to study should be investigated upon a basis of more definite measurements. Actual control sections must also be established by identical teaching material and procedure.

University of Pennsylvania

R. A. BROTEMARKLE

COCKS, A. W. *The Pedagogical Value of the True-False Examination*. University of Research Monographs, 1928, No. 7. Pp. 131.

The investigation reported attempts to measure the relative value of the true-false examination to the "teacher at work in the class-

room." The purpose of the study is three-fold—as to measurement, motivation, and instruction.

The immediate value of the true-false examination is said to be related to its form, procedure and scoring. Three first-year classes at the high school level were divided into two equivalent groups on the basis of mental age determined by the British Columbia Intelligence Test. The testing material consisted of an examination composed of two parts. Test 1 made up of fifty multiple choice or best-answer questions; and Test 2 made up of fifty true-false questions. Questions were based on the High School Physics course prescribed by British Columbia and the questions were divided proportionately in both tests. The pedagogical value of the true-false test is found to be more than 50 per cent greater than that of the multiple choice. A complete reëxamination substantiated the original finding.

The author then attempts to estimate the relative value of true-false statements. He adds his opinion to that of others and concludes that the false questions are of some value even when not corrected by the student.

He finds that while the multiple choice test is of greatest benefit to the bright student, the true-false test is of equal influence among the dull and bright.

Similar results are secured for tests in spelling, algebra and chemistry. He concludes from his own study and that of others that the best method of scoring true-false examinations is to instruct the pupil "not to guess" and inform them that the score "will be right minus wrong"; except when relative standing alone is wanted, in which case the right score alone is sufficient.

The pedagogical value most emphasized, that of review, is not inherent in the form or the material or the test itself, but is secured through the process of correction at the hands of the pupil.

R. A. BROTEMARKLE

University of Pennsylvania

VAUGHAN, W. T. *The Lure of Superiority*. A Study in the Psychology of Motives. N. Y.: Holt, 1928. Pp. 307.

Accepting Adler's theory of Organ Inferiority and Compensation and adjusting the same to Nietzsche's "will to power" the author has developed "compensation" as the "resolution of conflicting impulses." Considerable importance is attached to the time sequence.

Two general basic types of adjustment are the physiological with its process of substitution of another function, and the psychological with its substitution in kind. Specific types, based upon the "organ" theory and types of defect, are the physical, mental, social and moral. These latter may be basically of either general type, with the resulting direct or indirect functional activity.

Illustrative materials are drawn from the social groups in which conflicts have been intensive and persistent. The lives of Schopenhauer and Lincoln are used as individual illustrative studies.

The author has collected the problems of "inferiority" and "compensation" without in any way solving either; and in fact without the clarity sometimes used by the original writers. One is startled by the credence placed in statements and interpretations found in editorial and reportorial comments of certain of our modern newspapers and weekly digests.

R. A. BROTEMARKLE

University of Pennsylvania

LEARY, D. B. *Modern Psychology, Normal and Abnormal. A Behaviorism of Personality.* Phila.: Lippincott, 1928. Pp. 441.

The author states that his purpose is to "first, present a sober and scientific basis for the study of the behavior of personality"; "second, to deal with, evaluate and classify personality on the basis of types and degrees of dynamic adjustment to the physical and social environment; finally, bridge the gap between physiology, as an atomism of human behavior, to such a synthetic point of view as may properly be called a behaviorism of personality."

One is astonished at the variety of new terms, phrases, and theories necessary to the unity of the "sober and scientific" viewpoints. This necessity arises from a constant emphasis on "synthesis" as contrasted with the "all too-prevailing barren atomism of current behaviorisms."

A new theory of animal and human learning is advanced on the basis of the "general theory of the conditioned response." A new scheme of personality classification is based on a "dynamic-evolutionary theory of personality," and established subdivided groups of adjustment types centering about "non-association," "compensation," and "surrender" or "the inhibited exercise of the socially non-adjustive patterns."

Part IV, under the heading of "Types of Personality Adjust-

ment " presents a new viewpoint of religion and philosophy in terms of the synthesis or integration of human behavior. "Sharp and definite lines of demarkation" are evaded to such a degree as to leave little definite in the new interpretation. Mysticism becomes behaviorism.

R. A. BROTEMARKLE

University of Pennsylvania

ORATA, PEDRO TAMESIS, *The Theory of Identical Elements*, etc. Columbus: The Ohio State University Press, 1928. Pp. xi+204.

The author announces that his study is a critique of the theory of identical elements as employed by Thorndike to explain transfer of training and that it also sets forth a reinterpretation of transfer which is felt to be in better accord with the facts. The monograph is mainly philosophical rather than scientific in spirit. Orata has a reason for hoping that the theory of identical elements is erroneous and he does not hesitate to make that hope plain. He feels that, if the theory were sound, educational practice would be called upon to move back to the apprenticeship system which is, in his eyes, incompatible with the democratic ideal. It is a little difficult to understand just what democracy has to do with the validity of a scientific theory, but, of course, if one's attitude is not strictly scientific, one may, in the interests of certain social values, hope for the downfall of an embarrassing theory.

Orata reviews a considerable number of experiments and finds that transfer is dependent not so much upon the number of elements in common as it is upon the possibility of the subject being aware of some general principle applicable to the related activities. He believes this contention to be supported by the fact that Judd, Ruger, Ruediger, Coxe, Woodrow, Meredith, and others, who provided for such consciousness of general principles, tended to get more positive evidence of transfer than did those who worked with activities at an automatic level. He sums up his review of experimental evidence by saying: "We have demonstrated by the results of experimental investigations that transfer takes place to the extent in which conditions favorable to transfer are present" (page 157). Apparently he feels that this statement contains the death sentence for the doctrine of common elements. It is difficult for the reviewer to see that it is of the least consequence for the quarrel at hand. Even Thorndike has probably suspected that transfer takes place when conditions are favorable for the occurrence of that phenomenon.

The author's reinterpretation is based largely upon an arbitrary act of definition. He says (pages 175-176): "We have assumed that there are two types of behavior or reaction corresponding to two kinds of environment. One is automatic, reflex, or mechanical habit, in response to an unchanging environment; and the other is intelligent behavior in a meaningful and highly modifiable situation. In our problem of transfer we are concerned only with the second type of behavior (intelligent) and the second kind of environment (meaningful)." Is it any wonder, when the author admits that he will call transfer only such transfer as occurs where there is conscious understanding of relationships, that he will find transfer only under such conditions? The dice are loaded; the victory is sure.

There are few topics in psychology that are more deserving of critical examination than the transfer of training. Its importance is at once fundamental and practical. The doctrine of common elements is only an approach to the problem. Two activities can have anything in common from neurones to mathematical principles. What is needed is investigation of the degree of effectiveness of various types of common elements. Vague charges that this approach is based upon psychic atomism and mechanism or that it is against the interests of democratic education get us nowhere.

EDWARD S. ROBINSON

Yale University

NOTES AND NEWS

THE election committee of the American Psychological Association has announced the election of the following officers: President for 1930, Herbert S. Langfeld; Directors, 1930-1932, John F. Dashiell and Arnold Gesell; nominees for appointment to the Division of Anthropology and Psychology of the National Research Council, 1930-1932, A. T. Poffenberger and Calvin P. Stone; representative on the Social Science Research Council, 1930-1932, John E. Anderson. Carl C. Brigham of Princeton University continues as Secretary and Edward S. Robinson of Yale University continues as Treasurer of the American Psychological Association.

THE Seventh International Congress of Philosophy will meet September 1-5, 1930, at Oxford, England. Communications should be addressed to the Secretary of the Congress, Mr. A. H. Hannay, 74, Grosvenor Street, London, W. 1, England.

THE First International Congress on Mental Hygiene will be held at Washington, D. C., May 5-10, 1930. Communications should be addressed to the Administrative Secretary, Mr. John R. Shillady, 370 Seventh Avenue, New York, N. Y.

EDITORIAL NOTICE

BEGINNING with the 27th Volume in January, 1930, there will be a change in the dates of publication of the PSYCHOLOGICAL BULLETIN. In the future, the BULLETIN will be published only ten times each year (the August and September numbers will be omitted) with an average of 72 pages per issue. Thus the present size of the volume of 720 pages per year will be maintained by increasing the size of the separate issues.

THE PSYCHOLOGICAL BULLETIN

REPORT OF THE SECRETARY OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

BY CARL C. BRIGHAM

Princeton University

At the annual business meeting of the American Psychological Association, Inc., held in New York City on December 27, 1928, the following motion was adopted: On the recommendation of the Council of Directors, inasmuch as the American Psychological Association has voted to omit the annual business meeting in 1929, those portions of the By-Laws relative to the annual meeting be suspended for the year 1929 and that the Council of Directors be empowered to proceed with the election of new Members and Associates; in passing the budget for 1930, in making the arrangements for the 1930 meeting of the Association, with receiving reports of committees and with any other routine business of the Association which may come before it and that the minutes of this meeting shall be published as usual in lieu of the Proceedings of the annual meeting.

In accordance with this motion, a meeting of the Council of Directors was held on September 7, 1929, in Room 33, Kent Hall, Yale University, New Haven, Connecticut, at 2:15 P.M. Present were the President, the Secretary, the Treasurer, and Professors Allport, Dallenbach, Miles, Peterson, Weiss, and Woodrow.

Upon a motion duly made and seconded, it was voted that the minutes of the meeting of the Council of Directors held in New York, N. Y., on December 26, 1928, be approved.

Upon a motion duly made and seconded, it was voted that the minutes of the Thirty-Seventh Annual Meeting at New York, N. Y., be approved as printed.

The Council of Directors received the informal report of the Business Manager of the Psychological Review Company, including a financial report of the receipts and expenditures of the company from December 1, 1928, through August 31, 1929.

Upon a motion duly made and seconded, the Secretary was authorized to secure legal advice for the Council in reference to the dissolving of the Psychological Review Company, the expenditures for such legal advice not to exceed the sum of two hundred dollars.

Upon a motion duly made and seconded, it was voted that in view of the rapid increase in size of the American Psychological Association, Inc., a committee be appointed by the President to consider recommendations for the more effective management of the business affairs of the Association. The President appointed the Secretary, the Treasurer, and the Business Manager of the Psychological Review Company.

The Council of Directors received the report of the Editorial Board of the Psychological Review Company presenting the following recommendation: "That the Psychological Index be continued at least a year longer."

Upon a motion duly made and seconded, it was unanimously voted that the Psychological Index be not discontinued.

The following communication having been received from the Editorial Board of the Psychological Review Company, "That a 'classified author index' be included in No. 13 of the current volume of the Abstracts and that if this prove an acceptable substitute for the Psychological Index, the latter be forthwith discontinued," and the Council of Directors not finding itself in sympathy with this recommendation, upon a motion duly made and seconded, it was voted that no added funds be appropriated for the proposed classified author index of the Psychological Abstracts.

Upon a motion duly made and seconded, it was voted to accept the report of the Editor of the Psychological Abstracts consisting of a financial statement of the receipts and expenditures of the Business Office of the Psychological Abstracts from January 1, 1928, to December 31, 1928, audited and found correct by H. C. Warren and E. G. Wever on January 31, 1929, and a statement of the receipts and expenditures of the Editorial office of the Psychological Abstracts from January 1, 1928, to December 31, 1928, audited and found correct by J. P. Nafe and V. Jones on February 3, 1929.

Upon a motion duly made and seconded, it was voted to ratify

the previous mail vote of the Council nominating F. L. Wells to the **Executive Committee** of the Committee on Organization of the First International Congress on Mental Hygiene.

Upon a motion duly made and seconded, it was voted to ratify the nomination by the President of Professor H. A. Carr to represent the Association on the National Research Council's Advisory Committee to the Chicago Centennial Association to prepare plans for the Exposition in 1933.

The Secretary, with deep regret, announced the death of Edgar Pierce on February 15, 1929, of Edward F. Buchner on August 22, 1929, and of Morton Prince on August 31, 1929.

The Council received an informal report of the Treasurer for the period December 15, 1928, to September 3, 1929.

Upon a motion duly made and seconded, it was voted to accept with deep appreciation expressed on behalf of the American Psychological Association, Professor Howard C. Warren's generous gift of sixty shares of stock in the Psychological Review Company.

Upon a motion duly made and seconded, it was voted that the next annual meeting of the American Psychological Association, Inc., be held at the State University of Iowa, Iowa City, Iowa, on December 29, 30, and 31, 1930.

Upon a motion duly made and seconded, Professor C. E. Seashore was elected a member of the Executive Committee for 1930.

Upon a motion duly made and seconded, Professors Buford Johnson and S. W. Fernberger were elected members of the Program Committee for 1930.

Upon a motion duly made and seconded, Professors R. P. Angier and G. F. Arps were elected as representatives of the American Psychological Association, Inc., on the Council of the American Association for the Advancement of Science for the term 1930-31.

Upon a motion duly made and seconded, the following budget was adopted for the year 1930:

AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

BUDGET FOR 1930

Printing and supplies.....	\$500.00
Postage and express.....	400.00
Reprints	200.00
Year Book	600.00
Proceedings (Abstracts).....	400.00
Incidentals, 1930 meeting.....	60.00
Apparatus exhibit	50.00
Treasurer's bond and safety deposit box.....	12.50
Committee on Animal Experimentation.....	50.00
Subscriptions to Psychological Abstracts.....	3,300.00
Lawyer's fees	200.00
Secretary's stipend	1,000.00
Treasurer's stipend	100.00
	\$6,872.50

Upon a motion duly made and seconded, it was voted to extend to Yale University, on behalf of the American Psychological Association, an expression of its appreciation of the entertainment extended to foreign and American psychologists at the Ninth International Congress of Psychology.

Upon a motion duly made and seconded, the following Associate Members were transferred to Membership in the American Psychological Association, Inc.:

1. Luton Ackerson
2. Arthur G. Bills
3. Sheldon Glueck
4. John A. McGeoch
5. Samuel Renshaw

Upon a motion duly made and seconded, the following was elected to Membership in the American Psychological Association, Inc.:

1. Hudson Hoagland

Upon a motion duly made and seconded, the following were elected to Associate Membership in the American Psychological Association, Inc.:

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| 1. Frederick J. Adams | 5. Ernst A. Atkinson |
| 2. James B. Anderson | 6. Harriet S. Babcock |
| 3. Eston J. Asher | 7. Josephine Ball |
| 4. Robert C. Astrop | 8. Key L. Barkley |

9. Sarah J. Bassett
10. Arthur K. Beik
11. Hugh McK. Bell
12. Irving E. Bender
13. Robert G. Bernreuter
14. John E. Bohan
15. Marian Braungard
16. Cecil R. Brolyer
17. William A. Brownell
18. Mildred E. Burlingame
19. Norman Cameron
20. Burton M. Castner
21. Dwight Chapman, Jr.
22. Matthew N. Chappell
23. Florence M. Chitester
24. Siegen K. Chou
25. Glen U. Cleeton
26. Louis H. Cohen
27. Stephen M. Corey
28. James D. Coronios
29. George W. Crane
30. Albert B. Crawford
31. Mason N. Crook
32. William J. Crozier
33. Hazel M. Cushing
34. Frances C. Cutujian
35. Bonnye E. Deal
36. Henry N. De Wick
37. Elmer F. Diggins
38. Wilma T. Donahue
39. Mary L. Dougherty
40. Oscar Berry Douglas
41. Lewis E. Drake
42. Daniel D. Droba
43. Clifford F. Duncan
44. Howard Easley
45. Harold A. Edgerton
46. Merle H. Elliott
47. Alvin C. Eurich
48. P. Harry Ewert
49. Leo B. Fagan
50. Warren G. Findley
51. Audrey M. S. Firkins
52. Adelbert Ford
53. Clarence C. Fowerbaugh
54. Edward E. Franklin
55. Graydon La V. Freeman
56. Helen M. Gardner
57. Harold V. Gaskill
58. Louis W. Gellermann
59. Minnie Giesecke
60. Etta C. Gillman
61. Clarence H. Graham
62. Virginia T. Graham
63. Edward B. Greene
64. Pearl F. Gridley
65. Ella M. Hanawalt
66. Luberta M. Harden
67. Genevieve L. Harter
68. Richard S. Harter
69. Ingvald B. Hauge
70. Seth E. Haven
71. Joseph W. Hawthorne
72. Thomas A. Hendricks
73. R. Yorke Herren
74. Lovic B. Herrington
75. Ernest R. Hilgard
76. Richard S. Hill
77. Ching-Ju Ho
78. Rex Livingston Hoke
79. S. Daniel House
80. Thomas H. Howells
81. I. Huang
82. Doncaster G. Humm
83. Harry F. Israel
84. John M. Jacobsen
85. William T. James
86. Arthur F. Jenness
87. Dorothea E. Johannsen
88. Granville B. Johnson
89. Florence Justin
90. Lotta M. Karpeles

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|---------------------------|----------------------------|
| 91. Daniel Katz | 132. Vernon L. Miller |
| 92. Louis W. Keeler | 133. Alexander Mintz |
| 93. E. Lowell Kelly | 134. Grace Moore |
| 94. Agnes T. Landis | 135. Herbert Moore |
| 95. Delia L. Larson | 136. Elizabeth H. Morris |
| 96. Daniel B. Leary | 137. Emiline R. Moul |
| 97. Mary A. M. Lee | 138. Alfred D. Mueller |
| 98. Robert W. Leeper | 139. Karl F. Muenzinger |
| 99. Chester E. Leese | 140. Norman L. Munn |
| 100. Harvey C. Lehman | 141. Joseph W. Nagge |
| 101. Vernon W. Lemmon | 142. Bert Allen Nash |
| 102. Olive P. Lester | 143. Amalie K. Nelson |
| 103. Clarence J. Leuba | 144. Victor H. Noll |
| 104. David M. Levy | 145. Kermit W. Oberlin |
| 105. George A. Lewis | 146. Richard A. C. Oliver |
| 106. Kate Lewis | 147. Katharine T. Omwake |
| 107. Rensis Likert | 148. Sinforoso G. Padilla |
| 108. Edward A. Lincoln | 149. Richard M. Page |
| 109. Erich Lindemann | 150. Gladys E. Palmer |
| 110. John A. Long | 151. Emily Patterson |
| 111. Howard P. Longstaff | 152. Gaige B. Paulsen |
| 112. Elizabeth E. Lord | 153. Helen Peak |
| 113. Roger B. Loucks | 154. Glenn R. Pease |
| 114. Inez V. Love | 155. Laurence A. Petran |
| 115. Clyde A. Lynch | 156. Mary L. Phares |
| 116. Norman R. F. Maier | 157. Helen J. Reed |
| 117. Edith B. Mallory | 158. William P. Reed |
| 118. Carlyn J. Manasses | 159. James A. W. Reeves |
| 119. Anna Rachel Markt | 160. Martha M. Reynolds |
| 120. Dorothea M. Marston | 161. William W. Rogers |
| 121. Paul E. Martin | 162. Dorothy Rowell |
| 122. Kathryn E. Maxfield | 163. Beatrice R. Rubin |
| 123. Barbara A. Mayer | 164. Phillip J. Rulon |
| 124. John H. McFadden | 165. Edward A. Rundquist |
| 125. John M. McGinnis | 166. Metta M. Rust |
| 126. Roland C. McKee | 167. Maxwell L. Sacks |
| 127. Clarence L. McKelvie | 168. Matthew M. R. Schneck |
| 128. Charles B. McMullin | 169. Theodore C. Schneirla |
| 129. Quinn McNemar | 170. Gregory J. Schramm |
| 130. William H. Mikesell | 171. Adelin W. Scott |
| 131. Lawrence W. Miller | 172. Louise C. Seibert |

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| 173. Lowell S. Selling | 198. Eliza Ruth Valentine |
| 174. Georgene H. Seward | 199. Dorothy Van Alstyne |
| 175. Clarence H. Smeltzer | 200. William L. Van Buskirk |
| 176. Geraldine F. Smith-Seiler | 201. Noel B. Van Wagenen |
| 177. Randolph B. Smith | 202. Walter C. Varnum |
| 178. Donald S. Snedden | 203. L. N. Vernon |
| 179. Alto L. Snell | 204. Katherine Vickery |
| 180. Herbert Sorenson | 205. John Volkmann |
| 181. Earl B. South | 206. Thelma Grady Voorhis |
| 182. Ethel St. Clair | 207. Ruth W. Washburn |
| 183. Elizabeth M. Stern | 208. George E. Weigand |
| 184. Mabel E. Stewart | 209. James D. Weinland |
| 185. Stuart M. Stoke | 210. Persis E. White |
| 186. Robert Stone | 211. Carroll A. Whitmer |
| 187. Francis C. Sumner | 212. Lester E. Wiley |
| 188. Charles W. Telford | 213. Gertha Williams |
| 189. F. L. Templeton | 214. Edmund G. Williamson |
| 190. Wellington A. Thalman | 215. Erich C. Wohlfahrt |
| 191. Lorin A. Thompson, Jr. | 216. Cary C. Wood |
| 192. Ruth H. Thomson | 217. Horace G. Wyatt |
| 193. Burton D. Thuma | 218. Joseph G. Yoshioka |
| 194. Samuel Tomkinson | 219. Clarence W. Young |
| 195. Charles K. Trueblood | 220. Robert A. Young |
| 196. Austin H. Turney | 221. Kwang S. Yum |
| 197. David Turteltaub | 222. Thornton W. Zeigler |

The meeting adjourned at 11:30 P.M.

SUPPLEMENTARY REPORT OF BUSINESS TRANSACTED BY THE
COUNCIL OF DIRECTORS, SEPT. 9 TO DEC. 31, 1929

The Council of Directors received and voted to accept the report of the Business Manager of the Psychological Review Company showing all receipts and expenditures from December 1, 1928, to November 30, 1929, and ordered the report to be filed with the Secretary.

The Council of Directors received and voted to accept the report of receipts and expenditures of the *Journal of Abnormal and Social Psychology* from April 1, 1929, to November 30, 1929, and ordered the report to be filed with the Secretary.

The Secretary reported to the Council of Directors the resignations of J. W. Barton, D. P. MacMillan, G. E. O'Brien, H. V. Race, A. A. Roback, J. E. Russell, F. C. Sharp, W. T. Shepherd, R. B. Teachout, F. Thilly, and L. A. Weigle.

The Council of Directors received the report of the Committee on the Election of Officers as follows:

President for 1930: Herbert S. Langfeld.

Directors, 1930-1932: John F. Dashiell and Arnold Gesell.

Nominees for appointment to the Division of Anthropology and Psychology of the National Research Council, 1930-1932: A. T. Poffenberger and Calvin P. Stone.

Representative on the Social Science Research Council, 1930-1932: John E. Anderson.

The Council of Directors received and accepted the report of the Treasurer for the year 1929 as printed below:

AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

REPORT OF THE TREASURER FOR THE YEAR 1929

Dr.

To Balance from previous year.....	\$6,827.29	
Dues received from members.....	8,881.55	
Sale of Monographs.....	6.90	
Sale of Year Books.....	34.25	
Sale of used equipment.....	25.00	
Interest.....	67.94	
Total.....		\$15,842.93

Cr.

To Printing and supplies.....	\$375.68	
Postage and express.....	115.32	
Reprints.....	76.28	
Year Books.....	413.76	
Proceedings (Abstracts).....	86.69	
Incidentals, 1928 meeting.....	45.00	
Treasurer's bond and safety deposit box.....	12.50	
Payment on Review Publications.....	1,000.00	
Interest on notes, Review Publications, 1929.....	150.00	
Refund of certification fees.....	35.00	
Committee on Social Science.....	10.52	
Subscription to Psychological Abstracts.....	2,385.00	
Secretary's stipend.....	1,000.00	
Treasurer's stipend.....	100.00	
Total.....		5,805.75
Balance on hand.....		\$10,037.18

EDWARD S. ROBINSON,
Treasurer

Audited and found correct by

CLARK L. HULL and
S. M. NEWHALL

THE DIMMING EFFECT

BY THEODORE KARWOSKI¹

Harvard University

The "dimming effect" is a relatively unfamiliar phenomenon to psychologists and even to many students of the problems of vision. For this reason it seems desirable to direct especial attention to it, and to give a brief account of the history of its investigation.

The dimming effect has been described in standard experimental textbooks and has been casually referred to in the general discussions of visual theory. Titchener (16, 45) included the dimming effect under the caption, "Observation of the negative after-image with the persistence of the stimulus." Sanford (9, 113) discussed it under the topic, "After-images upon a background faintly tinged with the stimulating color." These authors stated that the effect is complementary to the hue of the pre-dimming color.

Ladd-Franklin (10, 485) offers the following experiment from Hering as a striking illustration of the dimming effect: "Put a bright red paper box, held vertically in the hand, in such a way that a strong light falls upon the paper. After a few moments' fixation, turn around; the paper will now be shadowed by the walls of the box and the difference in illumination is enough to cause the paper to be spread over with a bright layer of the complementary color. That is, the after-image of the first bright impression is strong enough to blot out completely the actual red paper, although it is still looked at with open eyes, and in a not too faint illumination." Criticizing the Helmholtz theory of after-images which are conceived of as responses of the relatively unfatigued retinal excitation processes to the self light of the retina, enhanced by illusions of judgment, Ladd-Franklin (11, 398f.) says: "There is a simple experiment of Hering's, also, which ought to become classical, in which the amount of after-effect is shown in a way which cannot be improved upon: one has only to look fixedly upon a patch of color illuminated by the light of a lamp, when the patch, still looked at with open eyes, will appear to be spread over with the complementary color. This shows without question that the after-effect is of very

¹ Fellow of the National Research Council.

considerable intensity, and that some very pronounced physiological process is taking place in the retina."

An instance which shows how slight the changes of conditions may be to elicit the dimming effect is cited by Edridge-Green (4, xiv): "A vivid blue-green after-image may be seen not only in the absence of all green lights but whilst the eye is still being stimulated by a red light. If in a dark room in which all extraneous light has been excluded an incandescent light be covered by a deep red glass and a purple glass combined the only light transmitted will be a red light in the neighborhood of the A line. If the filament of the light be attentively regarded for twenty seconds and then the eye be slightly moved a bright blue-green after-image will be momentarily seen though the eye is still stimulated by red light."

Ebbinghaus (3, 238) gave the following description: "Suppose one fixates for a time a red slide on a yellow ground, and then reduces the objective light intensity of both colors by holding opaque cards in front of the window side of the room or reducing the light of the lamp. Fixation should be kept constant. One then perceives through the shadows, as if it were under-illuminated, an experience of great beauty—a green film on a blue ground. One has the impression as if with every excitation in the eye of a certain color sensation, there accompanies, at the same time, an active production of the complementary color. As long as the stimulus endures in its original intensity, this accompanying effect gradually increases, and more and more reduces and finally neutralizes the original sensation: this is adaptation. When the stimulation is neutralized or merely sufficiently reduced in intensity, the complementary color, meanwhile collected, so to say, steps forth clearly and completely, and this is the negative after-image."

In 1854, Scoresby (15, 546) reported the following observations in viewing spectral lights: "Sometimes the smallest change in the light was found to alter greatly the character of the spectrum. In certain cases the compressing of the eyelids, or the mere passing of the hands between the eyes and the light, could serve to change the negative pictures, or colors as viewed in the usual way, into their complementary tints." What seems to be an early reference to the dimming effect is that of Lehot (12), who in 1830 directed special attention to the color phenomena produced by suddenly varying the distance of the colored field.

Investigators have frequently applied the dimming effect procedure in order to isolate some particular aspect of visual experience.

McDougall (9, 379) sought to prove that yellow is a complex color containing red and green components: "On diminishing the illumination of a patch of Y after fixating it for thirty seconds or more, it usually becomes R unless the period of fixation has been considerably prolonged, when it usually becomes B." Hauer (7, 648) dimmed white light and observed an effect which matched a spectral light of 460μ . This bluish phenomenon was attributed to the natural light of the rods as a consequence of the reduction of the intensity of the stimulus below the cone threshold. Forbes (5) suggests a method for standardizing white light by checking for chromatism upon dimming.

To Hering (8, 132, 40) the dimming effect was merely a temporary reversal of the state of the dissimilative and assimilative processes of the retina. As such he considered it to be a pretty demonstration of the fundamental bases of his theory of vision. Although Hering never felt that the dimming effect is a phenomenon essentially different from the ordinary complementary after-image, he was interested in the richly saturated color effects so readily produced by this method. Some of the most striking ways of demonstrating the effects were first reported by Hering. If Hering did not see any difficulty in incorporating the dimming effect in terms of his theory of vision, he considered it quite unexplainable by the Young-Helmholtz postulates. Indeed, his most important arguments against the Young-Helmholtz theory of simultaneous contrast utilize the principle of the dimming effect. To Hering's last attempt at determining the contrast or antagonistic colors of visual experience, we probably owe the introduction of the dimming effect as a method of experimental procedure. The determination of such colors was intended to establish the simple color sensations of vision.

Hering's use of the dimming effect as a methodological instrument is shown in the work of Dittler and Satake (1, 240). This investigation was performed under the direction of Hering, who wrote the introduction to the paper. The work illustrates the delicacy of the dimming effect when applied as a means of measuring the accuracy of mixing colors to produce white. Since it is one of the first carefully conducted researches utilizing the dimming effect, it is appropriate to report this paper in detail.

Students of vision know that Hering has always insisted on maintaining a distinction between complementary and antagonistic colors. Originally, Hering used the term complementary only with reference to the Young-Helmholtz theory or theories of similar kind. Thus,

"mixed lights appear colorless when they act on the blue-yellow or the red-green substance, with equal dissimilative and assimilative power, for then both effects neutralize each other, and the action of the white-black substance alone remains. For this reason, two objective kinds of light, which give white when mixed, are not complementary but antagonistic; they do not produce white by combinations, but merely destroy each other and leave visible the white which was already there."

In the introduction to the Dittler and Satake paper, Hering distinguishes between the complementary and antagonistic (*Gegenfarbigen*) colors in this manner: "One determines complementary colors by equating an apparent white (that is, the white may merely appear to be white) and the white caused by two different wave lengths. These two wave lengths are complementary to the composite color but they are not necessarily antagonistic colors because, according to the theory (Hering), two homogeneous lights are only *Gegenfarbig* stimulant values, if when mixed in proper proportions they give a white to the CHROMATICALLY NEUTRAL EYE. In a room illuminated by daylight, the eye is always chromatically out of tune, because the daylight which reaches the visual epithelium possesses, besides the white stimulant value, also a colored one. That we always see white in natural or artificial illumination depends upon the fact that the eye can adapt itself to any kind of general illumination—not only brightness but color-toned illumination as well. Therefore, white things, in spite of the existing color disharmony within the eye, always appear white. If one shuts out the said light from the retina, after-images of previous illumination disappear and finally the visual field is seen as *Eigenlicht*. Then the eye is chromatically neutral but more sensitive to white. If now a white light impinges upon the retina which possesses besides a white a small color value—the color component may be so small as to be unnoticeable—and if one fixates an illuminated field in such a light for a long time and then suddenly reduces the intensity of the light, the chromatic disharmony betrays itself, because the colored stimulus value it contains shows a definite hue which is the *Gegenfarbe* of the color corresponding to the stimulus value of the subdued light stimulus."

Hering proceeded to say that by this method of dimming lights it is possible to discover mixtures of white which will show no noticeable chroma upon dimming, and thus the simple *Gegenfarben* as well as the complementary acting lights of daylight equations may be determined. "As there are numerous instances of literature on

complementary or homogeneous lights which in proper mixture give equations with daylight, gas, or electric light, and since the homogeneous lights of the *Gegenfarbig* effect, apart from my occasional observations, have never been directly determined, I have induced Messrs. Dittler and Satake to determine a series of lights affecting the eye in a *Gegenfarbig* manner, according to the theory above outlined."

Dittler and Satake determined the *Gegenfarbigen* wave-lengths. Then used a small telescope, the visual field of which could be flooded with light. This field of light, or sections of it, could be darkened by blinds pushed into the ocular. A fixation point was maintained in the center of the field. The stimulus fell on the extra-macular region of the retina, because foveal stimulation was prevented by a dark disk. The retinal image was 2 mm. in diameter. The inner margin of the semi-circular stimulus was 1 mm. from the center of the fovea. The experimenters used eleven binary light mixtures, of which the long wave-components were between $665\mu\mu$ and $566.2\mu\mu$. The use of lights outside of these limits proved unsafe, since the extreme limits of the spectrum would be involved. In the search for the *Gegenfarben* the experimenters always started with the long wave-lengths, which were kept at constant brightness, while they searched for the suitable short wave-length in respect to wave-length and brightness. When wave-lengths were found so that they mixed to give a white, to the dark-adapted eye, they were checked to discover whether they are really tone free by dimming the mixture after pre-exposure to the retina for thirty seconds. Testing mixtures was continued until tone-free, or rather relatively tone-free, effects were obtained. The contrast effect on dimming readily indicated the direction in which improvement could be made.

These investigators modified the above experiment in order to determine the complementary wave-lengths, as defined by Hering, for the same long wave-lengths used in determining the *Gegenfarben*. This was accomplished by flooding the unused half of the telescopic field with daylight and matching it with the already determined binary mixtures that were found to be tone-free. It was found that the two did not match. Daylight contained a decided yellow valency—so much so, that the already determined binary tone-free mixture seemed decidedly bluish by contrast with daylight. The experimenters proceeded to find a match by varying the short wave-length in respect to wave-length and brightness while the long wave component was maintained as a constant. When a match was established, the short

wave-length was noted and it determined the complementary color for the particular long wave-length used.

An important difficulty with this experiment is the fact that the values given for the *Gegenfarbig* effect are relative values. Actually, Dittler and Satake never found absolutely toneless results upon dimming. "A certain difficulty grew out of the search for *Gegenfarbigen* lights, according to the contrast (dimming) method, out of the fact that even with the most favorable choice of both lights a weak BLUISHNESS remains. But this bluishness was not comparable in intensity to that which was the result when we filled the aperture with daylight and dimmed after the same period of fixation. Why it was not possible in these questionable cases to eliminate the last amount of weak blue from the field of vision we have not been able to determine." This difficulty would not be anticipated by the fundamental laws of color mixture, since it has been generally held that proportional increase or decrease of each component in the mixture does not destroy the match.

The attempt of Dittler and Satake to determine the Hering *Urfarben* by the dimming effect method has been recently renewed by Schubert (14). This research was particularly concerned with the problem of the Urrot and the Urgrün. The failure of these primary colors to be complementary has been considered a serious objection to the Hering conception of color vision. Now Schubert finds that the Urrot and Urgrün are in fact complementary to a chromatically neutral and foveally stimulated retina. This is experimentally demonstrated by producing a white upon mixing the previously determined Urgrün, 503μ , and Urrot, 670μ plus 451μ . Moreover, this white does not betray a chromatic valence upon dimming. Schubert attempts to explain the failure of previous workers to obtain a white, on the grounds that the reported yellow is due to the chromatic effects caused by the adaptation of the eye to the chromatic elements inherent in the general illumination.

With reference to the dimming effect as a phenomenon of vision, it is interesting to note that under similar conditions of experimentation Dittler and Satake have reported bluish effects, whereas Schubert was able to find complementary colors that failed to reveal a chroma when their mixture was dimmed. Schubert seems to take it for granted that Dittler and Satake allowed a yellow-green valence to enter into their experiments, which is hardly justifiable, since their experiment was meticulously prepared to rule out the effects of previous retinal stimulation by the general light. However, it is not

clear whether Schubert is positive that no chromatic effects were experienced upon dimming white because he mentions that "it is often difficult to distinguish darkness from blueness" (page 92). A more sudden method of dimming than that used by Schubert might have brought out the chroma more clearly.

Unlike Hering, Troland (17, 84-88) is inclined to believe that the phenomenal aspect of the effect is difficult in explanation and so worthy of study. Troland worked with spectral lights and automatically reduced the intensity of a foveal stimulus by means of an electrical device. Eight spectral lights were investigated. Two trials were made for each eye with preexposure time of 32 sec. The lights had an approximately equal primary brightness of about 800 photons. The dimming was approximately 10 per cent. Three observers were in agreement with regard to the nature of the dimming contrast effect. "The exact color which appears with dimming depends upon the degree of adaptation and the amount of dimming, these two being more or less interchangeable. Thus with the standard red slight dimming gives a violet; then follow: purple, blue, greenish blue, and blue green, as the extent of dimming or adaptation is increased. The dimming colors observed for the eight standard spectral lights employed in the experiments described in this monograph were as follows: red, blue or purple; yellow, reddish; yellow-green, faintly violet-blue; blue-green, dark blue; blue, simply darkening; violet, darker and more saturated violet. With color filters effects were observed similar to those obtained with spectral lights. It will be noted that the dimming colors recorded above involve exclusively the chromatic components B and R, the B being decidedly predominant. In this connection it should be recorded that dimming of the stimulus afforded by a sheet of Hering blue paper in bright sunlight (6,700 meter candles and the natural pupil) yields a brilliant saturated red.

"Although the results recorded above do not lend immediate support to Hering's conception of the appearance of antagonistic colors through dimming, there are, nevertheless, striking phenomena observed during the dimming experiment which bear out Hering's general conception of color antagonism. For example, the failure of the G component to appear with the dimming of a red stimulus until the latter is practically extinguished may obviously be attributed to the antagonism between this component and the R excitation natural to the stimulus. The B easily comes to view because of the weaker Y valency of the given light: similar considerations apply to

the green stimulus; with very strong dimming the latter yields a violet or purple. The predominance of the blue may be explained by the idea—in favor of which there is much to be said—that the mechanism underlying the B component is much less rapid in its adaptation changes, or else has a higher equilibrium level than that of the other components. This conception is in harmony with Hering's general views."

In this work Troland also studied the effect of dimming white lights. "The occurrence of the dimming contrast effect with ordinary white light was verified by experiment with a number of subjects. All the whites employed, with the possible exception of sunlight on white drawing paper, showed marked chromatic effects upon dimming, but these varied not only with the length of the pre-exposure and the original or primary intensity of the stimulus, but with the observer." Troland also pointed out that his experiments indicated that preexposures of the stimulus for durations less than a second before dimming gave much richer and more prolonged effects than those of longer preexposure periods. Ordinarily such short exposures of stimuli to the retina are quite favorable for positive after-images, when dimming is practically complete or total as in the case of projecting after-images on gray backgrounds. Hence it is to be expected that positive after-images would be involved in dimming effects of short preexposures of the stimulus. Actually, "dimming the blue from the region of $451\text{--}455\mu\mu$ of the spectrum after a preexposure of one-eighth second and at 600 photons, the dimming effect was a positive, lasting 30 seconds, which was followed by a negative persisting 200 seconds. With spectral red under similar conditions, no effects of this sort were obtained." These latter observations were more or less of a casual nature but they are offered as suggestions to possibilities of research by the dimming effect procedure.

Having shown that the contraction of the pupil has much influence on the fluctuation of images, Troland (18, 368) pointed out that the dimming effect could be induced by the contraction of the pupil "as well as by turning down of the lamp or the closing of a shutter." He, then, applied this logical deduction to actual experiment and confirmed its truth in the case of fixating light spots of low intensity. The disappearance of the spot of light, upon the increasing adaptation of the eye, correlated with the moments of the contraction of the pupil.

In the paper on "The Influence of Changes of Illumination on

After-Images," Troland (19, 499f) devotes much of the space to the consideration of the dimming and brightening effects. The converse of the dimming is the brightening effect which is governed by laws similar to those conditioning the dimming effect. The qualitative difference is that the dimming contrast effect shows a tendency to be displaced towards the complementary hue or the negative after-image, whereas the brightening effect tends toward the positive. The effect in either case fades quite rapidly, being only in evidence during the change of stimulus conditions, and so Troland suggests that both kinds of effect depend on the lessened resistance to change of the stimulus by the fatigued parts of the retina.

By the method of changing the illumination, either brightening or darkening, Ebbecke (2) distinguishes between the visibility of the after-image and the presence of retinal after-stimulation. A theory in terms of the adaptation of the eye to its own after-images is proposed to account for the changes from positive to negative, and *vice versa*, that the after-images undergo as the results of change in the illumination.

A large mass of data concerning the dimming effect under abnormal conditions is available under the topic of Erythropsia. Much of the evidence is of a clinical kind. The phenomenon of Erythropsia or red vision seems to be experienced by patients after cataract operations. The sudden increased sensitivity of the retina to brightness in the case of these subjects causes them to see reddish purple when brightly illuminated rooms are exchanged for more subdued ones. Fuchs (6) showed that Erythropsia may be experienced by the normal eye under conditions of intense and prolonged glare of snow light. This paper is the most important experimental work on this subject, and has had considerable influence upon the theorizing of ophthalmologists.

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EXTROVERSION AND INTROVERSION

BY J. P. GUILFORD AND KENNETH WALTER BRALY

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No single pair of traits of personality has been quite so widely discussed and studied as that of extroversion and introversion. Originating with the psychoanalysts, with Carl G. Jung in particular, extroversion and introversion have been slowly stripped of the poetic terminology which has been used to describe them, and brought within the reach of experimental methods. Sometimes the distinction between them is a matter of the direction of interest of the individual; at other times it is a matter of readiness or facility of adaptation, especially to the social environment; again, the distinction is made on the basis of the balance or proportion of overt and covert behavior exhibited by the individual. The distinction, then, seems to have intellectual, emotional, and social aspects.

Psychological theories or definitions: No clear-cut classification of the theories of extroversion-introversion can be made. While they fall roughly into the three groups mentioned above, there is so much overlapping that it would be folly to attempt to classify them in that way. There is a general belief that the extreme forms of both extroversion and introversion are pathological, although not always equally so.

Jung (24) first suggested a classification of individuals into the two psychological types on the basis of the "flow of the libido." In the extrovert the flow of the libido is outward toward the object. The object contains the unconditioned value for the subject and it determines to a large extent his reactions. In the introvert the flow of the libido is inward from the object. The unconditioned value is in the subject. In the final analysis extroversion and introversion are not traits of personality, but rather they are *mental mechanisms* which can be turned on or off at will.

Not being able to classify all cases into these two types, Jung proposed four sub-types for each one (26). These sub-types are distinguished upon the basis of the function which plays the dominant rôle in the adaptation of the individual: sensation, feeling, thinking, and intuition. The dominance of any function is proportional to the

degree with which it is differentiated from the unconscious. This does not complete the picture for Jung. The types are cut across by the factor of compensation (26). If the individual is introvert in his conscious adaptations, he is extrovert by way of compensation, in his unconscious adaptations. If one of the four functions is very highly differentiated for conscious mental life, the balance is restored by a strengthening of the other functions in the unconscious mental life. Thus everything can be explained and one can have one's cake and eat it, too. The only support for these views consists in the clinical examination of neurotics and the post-mortem examination of literary and historical material.

Jung should not be given the entire credit for the origin of the extrovert-introvert dichotomy. As early as 1900, Stern (46) had suggested a pair of types known as "objective" and "subjective," which he found to differ in regard to simple reaction time under sensory and motor instructions and also in their reactions in the *Aussage* tests. Others who wrote later concerning these same two types are Klages (28) and Kurella (29).

Another writer previous to Jung, and one to whom Jung gives some attention, is Otto Gross (15). His two types, which were discovered in the field of pathology, were called the "deep-narrow" and the "shallow-broad." They were distinguished upon the relative amounts of "primary" and "secondary" function present, the primary functions being those which follow directly upon stimulation, and the secondary ones being those which persist after stimulation and permit organization and systematization of sense-impressions. Heymans and Wiersma (19) contributed factual data which were derived from interviews with 2,523 individuals and which bear upon the characteristics of these two types.

It is indeed difficult to establish priority in this as well as in other ideas in science. We find William James distinguishing between "explosive" and "obstructed" wills in 1890 (23). He describes his "tender-minded" and "tough-minded" types in 1907 (22). J. M. Baldwin (4) speaks of "sensory" and "motor" types in 1902. It does not require much inspection to find a great deal in common in all these writers.

Beatrice Hinkle (20) wishes to split off from the usual extrovert-introvert dichotomy another pair of tendencies. She adds what she believes to be another dimension of personality with her objective and subjective types at either extreme. These types are distinguished on the basis of the degree of differentiation of the mental

processes in general from the unconscious. The subjective type has the greater differentiation, and is given to complexities and subtleties which are missed by the objective type. Extreme subjective types exhibit instabilities.

William A. White (52) accepts Jung's definitions of the types, but he also emphasizes the abnormality, if not the pathology, of introversion. Introversion, for White, is "a lessened capacity for integration of the personality, of separating the self from the not-self." Thus, extroversion is allied to the process of individuation, while introversion is allied to the opposite tendency, or regression.

While there is little proof one way or another, many of these writers regard the traits as hereditary in nature. Jung, Hinkle, and Wickes, all insist upon the innate determination of them, and even venture the warning that a radical change of the tendency of an individual may have dire consequences. On the other hand, some writers admit that the fundamental basis of the traits may be hereditary, but believe that moderate changes can be made without harm. Among the latter group are Nicoll, Tansley, Freyd, McDougall, and Conklin.

Wells (51) conceives of introversion as an adjustment which avoids the unpleasant or the necessity of external effort. Satisfaction is rather sought within the self, in imagery and day-dreaming.

Allport and Allport (1) include extroversion-introversion among the traits of "self-expression." The distinction is made on the basis of the attitude of those whose images and thoughts are readily expressed in overt behavior and those who create an inward, imaginary world as a substitute for the real world. G. W. Allport (2) says that extroversion and introversion, like other traits, may be defined as independent, statistical variables; as a hierarchy of integrated specific habits or a dynamic trend of behavior resulting from such a hierarchy, and as a habitual mode of adjustment governing specific responses. He makes no choice among these interpretations.

June Downey (11) finds extroversion and introversion to be constellations of traits which are included in her analysis of will-temperament. Introversion includes slowness of movement and decision, inertia and tension, inflexibility, interest in detail, and volitional perseveration. Extroversion includes the converse of these tendencies.

Conklin (7) attempts to interpret introversion and extroversion in the language of academic psychology when he takes the conditions of attention as a basis for them. Introversion is the state of mind

in which attention is governed by subjective conditions and extroversion is the state in which attention is governed by objective conditions. He introduced the term "ambivert" to designate those individuals who retain a flexibility which permits fluctuation from the one state to the other. Something like a normal distribution is assumed and so the division of people into two separate and distinct types would disappear as a possibility.

Later, Conklin (10) considers introversion and extroversion as distortions of thought. Both are regarded as flights from reality or attempts to evade the issues of life, the former being a withdrawal within, an egocentricity, while the latter is a "flight into reality." The terms "para-introversion" and "para-extroversion" are suggested for these pathological forms.

Freyd (13) attempts to assimilate and to epitomize all definitions as follows: "Introvert: An individual in whom exists an exaggeration of the thought processes in relation to directly observable social behavior, with an accompanying tendency to withdraw from social contacts. Extrovert: An individual in whom exists a diminution of the thought processes in relation to directly observable social behavior, with an accompanying tendency to make social contacts." He has also collected a list of fifty-four descriptive behavior-traits from various authors, a list which has formed the basis for several rating scales since that time.

Physiological theories: While some of the above writers have hinted at a neurological or glandular basis for these traits, none of them have anything like a working hypothesis which is based upon bodily processes.

Kempf (27) was the first to suggest a neurological foundation for these tendencies. The underlying basis, according to him, consists in the relative development and dominance of the autonomic and central nervous systems. The introvert has a more highly developed and more dominant central nervous system, and hence he is more subject to inhibitions and delayed responses of a directly adaptive nature.

Marston, who was primarily interested in the study of emotions, regards introversion and extroversion as a matter of inhibited emotional expression, although he admits differences outside the sphere of emotion (36). "Extroversion is the skeletal expression of emotion; introversion is the dissipation of emotionally aroused energy within the organism rather than the adequate discharge of this energy through skeletal channels upon the environment." This theory is

essentially the same as that of Kempf; the two traits are correlated with the two systems, the central and the autonomic.

Ramsey Hunt (21), who considers the two fundamental processes of neural functioning to be excitation and inhibition, believes that the solution of types is to be found best in terms of these two processes. These coöperative, but opposing processes, by their balance, regulate all activities of the organism. He believes, moreover, that inhibition must be considered just as active a process as excitation, rather than merely as a negation of excitation. He proposes the name of "erethitic" for those individuals in whom inhibition tends to be the stronger process, and "kolytic" for those in whom excitation tends to be the stronger, but he recognizes that these two types are analogous to the introvert and the extrovert, respectively.

An outstanding theory of the types under consideration is that of McDougall (34). He interprets introversion and extroversion either as opposed tendencies of temperament, or as maximum and minimum points on a scale of a single positive factor which he believes is due to some general property of the nervous system. The probability is that this general property is of a chemical nature or a resultant of metabolic differences. The factual basis for the last statement is the marked effect of certain drugs upon these temperamental traits. An individual's position on the scale may be shifted toward the extrovert end by alcohol, ether, chloroform, and allied drugs, and toward the introvert end by strychnine, morphine, caffeine, and other alkaloids.

Another point of evidence is the fact that hypnosis can be induced easily in hysterics and cyclomanics, who may be considered extreme extroverts, while schizophrenics, who are extreme introverts, are hypnotized only with great difficulty, if at all. The drugs mentioned, moreover, seem to increase or decrease, respectively, the ease with which hypnosis may be induced, as if the expected shift toward the extrovert or introvert end of the scale, respectively, had been made.

These facts lead McDougall to suggest that extroversion is a matter of the tendency toward dissociation, and the physiological theory of dissociation in general is a heightening of synaptic resistances. Extroversion depends upon a heightened resistance of the synapses, and introversion upon a lowered resistance. The mark of the introvert is therefore greater activity of the higher brain centers and an inhibition over the lower centers. The essential mark of the extrovert is the ready transmission through the lower centers; action without inhibition.

Rating scales and tests: A number of rating scales and tests have

been devised and used in various ways. The tests must be regarded as very tentative, as yet, since none of them have had a very rigid trial, and the results which have been obtained from them must also receive suspended judgment. Nearly all of them depend upon subjective judgments, either of the rater himself or of others who have observed him.

One of the first scales, and the one which is probably most widely known, is that of Laird (31). His list of forty-one tendencies may be rated in ten steps or degrees ranging from extreme extroversion to extreme introversion, by the subject himself or by an associate. After giving the test to several thousand individuals, both in industry and in college, Laird concluded (32): that women tend to be more introverted than men; that youth tends to be more extroverted than old age; that there is little or no relation between introversion-extroversion and intelligence; that there are no constant racial differences in these traits; that foremen and executives tend to be distinctly extrovert and that office workers and research workers tend to be introvert.

At about the same time Marston attempted to develop both a rating scale and a more objective test based upon observed behavior (36). One hundred nursery school children were rated by three of their teachers on twenty pairs of opposed tendencies, most of them social in character. The behavioristic tests were designed to measure the five following factors: social resistance, compliance, caution, interest, and self-assertion. The test-situations were simple and natural, and the rating of the child's response depended upon arbitrary standards. Results from both tests and rating scales indicate that boys are more extrovert than girls and that there is an increase of introversion with advancing age. The correlations between the two kinds of measurement were of the order of .50.

Heidbreder (17), using the items of the Freyd list of some fifty behavior-tendencies, secured self- and associate-ratings of 200 students. A normal distribution curve was obtained for the group. The relative diagnostic value of each item of Freyd's list was determined. She repeated (18) the procedure with 200 new subjects and found all but six of the items highly diagnostic. She found no significant sex differences with this test.

Guthrie (16) compared six different tests with each other, four of which had been devised as tests of extroversion-introversion, using 265 students as subjects. The tests were as follows: an intelligence test; scholarship records; the Laird personal inventory, which he

found to have a reliability coefficient of .60; a campus-information or gossip test, with a reliability of .72; Jung's word-association test; and an instructor-rating test, in which the students judged their instructors. All inter-correlations were so small as to be regarded as negligible, which would indicate that the tests had little or nothing in common.

Travis found that the auditory threshold is raised during reverie in negativistic subjects and is lowered under the same condition for suggestible subjects, both normal and pathological (48). It would seem, because of the connection between extroversion and suggestibility, that this simple objective test would also be diagnostic of extroversion-introversion. Travis later tested fifty-nine pathological subjects (49) and found that every one of twenty-two patients classified as "psychoneurotic," for which he gives no specific definition, had a lowered threshold during crystal-gazing, and twenty-one of twenty-four dementia praecox patients had a raised threshold. Of the three latter cases who had a lowered threshold, two could be hypnotized, and the other one had been doubtfully diagnosed as dementia praecox. Two facts disqualify the threshold test as an acceptable criterion of extroversion-introversion. In the first place, we do not know that the psychoneurotic group was composed entirely of extrovert cases. In the second place, both of the two manic-depressive patients who were tested had a raised threshold and this group is usually regarded as extrovert.

Conklin (8) introduced a test which requires the subject to estimate the degree of agreeableness or disagreeableness of forty proposed activities. Its aim is to measure differences within the normal range of introversion-extroversion. The reliability of the test, determined by correlating halves of the test given to over 300 subjects, was .92. The correlation with the Laird test was .37, in spite of the fact that the Conklin test is restricted to the direction of interest or attention alone. Men proved to be more extrovert than women, as might be expected from the types of activities used in the test. Differences were found between different professional groups in the university and outside the university.

Campbell (6) applied the Freyd list to insane patients. Self- and associate-ratings correlated .37. She found that the test discriminates very well between the different forms of psychoses, but the numbers of cases are entirely too small to be significant.

McDougall (34) suggested a very simple and objective test for these opposed tendencies. Upon the two assumptions, (1) that

these two traits are distinguished by the degree of dissociation of the higher nervous centers, and (2) that the rapidity of the fluctuations of an ambiguous figure stands in an inverse relation to that degree of dissociation, he uses the frequency of alternation of a simple ambiguous figure, *e.g.*, a rotating windmill wheel, as an indicator. The experimental fact, based only upon three subjects (35), that the alcoholic group of drugs reduces the rate of fluctuation and that the alkaloid group increases the rate, in a measure justifies the use of such an indicator.

In some studies at Vassar (50), however, little or no agreement was found between McDougall's indicator of extroversion-introversion and Marston's rating scale for the same thing.

Bender (5) found a correlation of .379 between the Freyd list and Allport's ascendance-submission test when applied to approximately 400 students. Thus the grouping of these two pairs together, as Allport does in his analysis of personality (1), receives some experimental support. The fact that the two tests contain some nearly identical elements, however, should guarantee a certain degree of positive correlation.

Neymann and Kohlstedt (41) have recently devised a test which bears resemblance to both the Marston and the Freyd tests, especially to the latter. The fifty items of the test were selected from twice this number used with cases of insanity. One hundred patients displaying symptoms of schizophrenia and also 100 cyclomanic patients were used as subjects. A distinctly bi-modal curve resulted. The reliability of the test has not been established. The validity of it rests upon the fact that correct diagnosis of the two psychotic groups could be made in more than 93 per cent of the cases.

Physical correlates of introversion-extroversion: A number of studies have indicated that there is a physiological basis for these traits, or for traits of a related character.

One outstanding attempt to correlate psychological types with morphological types was made by Kretschmer (29). He calls the two psychological types "cycloid" and "schizoid," as they resemble the cyclomanic or the schizophrenic types of psychotics respectively. The two chief physical types are the "pyknic" or the compact, full-bodied type, and the "asthenic" or the narrow, thin-chested, slender type. He found a close correlation between the cycloid and the pyknic types, and, although the schizoids were of more varied physical types, they were most commonly of the asthenic group.

Mohr and Gundlach (37) studied eighty-nine native, white, male

inmates of the Joliet prison. They were classified into Kretschmer's three groups as follows: 19 asthenics, 26 athletic, and 44 pyknics. Nineteen psychological tests were applied. Wide differences were found between the asthenic and pyknic groups in at least nine of the tests. The asthenic group was found to be superior in the intelligence test, the information test, and in several tests of speed of reaction. The two groups were about equal in tests of attention and learning ability. The striking novelty of these results lies in the fact that speed of reaction has usually been attributed to the extrovert rather than to the introvert pattern.

Naccarati and Garrett (40) divide people into three physical types: the macrosplanchnics, in whom the trunk predominates over the development of the extremities; the microsplanchnics, in whom the limbs are more developed than the trunk; and the normosplanchnics, or the middle class. Kretschmer's work had been verified previously as far as the abnormals are concerned (39). With fifty-four normal students as subjects, a rather significant correlation was found between the body index and the Laird personal data test.

Bender (5) secured data which can be interpreted as verifying these relationships, the introvert being more microsplanchnic and the extrovert being more macrosplanchnic.

Furukawa (14) attempted to correlate temperament with blood group types. Two hundred sixty-nine school girls were used as subjects. The temperamental types were designated as "active" and "passive," but they may be readily identified with our extrovert and introvert, respectively. Using the isoagglutinin test, Furukawa found that the active group tended to fall in types I and III of the blood groupings, and the passive group in types II and IV. The correspondence was 81.7 per cent in the first case, and 79.5 per cent in the latter.

Rich (43) attempted to determine the relation of bio-chemical differences to personality. Chemical tests were made with fifty-seven students by measuring the acidity of urine, the presence of inorganic phosphorus, and of creatinine, and the hydrogen-ion concentration of the saliva. Ratings were obtained in good-naturedness, emotional excitability, perseverance, leadership, and aggressiveness. The results were confirmed in a study of 303 children. Rich's conclusions are: The least excitable individuals tend to have the most acid fluids, while the more excitable persons tend toward neutrality or alkalinity in their fluids. The least aggressive excrete the greatest amount of acid and have the highest alkali reserve in the blood. Emotional excitability tends to bear an inverse relationship to the creatinine in the

blood and urine. These conclusions are based on correlations between .20 and .30, which he thinks would probably be raised under more carefully controlled conditions, although these low coefficients may indicate the low share that body chemistry contributes to personality. Specific ratings of introversion-extroversion were not obtained, but the differences which were studied by Rich resemble, in a measure, the differences in those more inclusive traits.

McDougall and Smith (35) secured evidence of a chemical basis for extroversion-introversion. They found extreme differences in the rate of fluctuation of an ambiguous figure which corresponded to subjective estimations of extroversion-introversion. Only three subjects were tested, however. Drugs had a marked effect upon the rate of fluctuation, and the two groups, the alcoholic and the alkaloid, influenced the rate in opposite directions.

Conclusion: There is considerable agreement among psychologists upon the existence of a bi-partite distinction known as introversion-extroversion and there is some agreement upon its definition, although the intellectual, emotional, and social aspects of it are variously emphasized. Tests and rating-scales for the trait have in general an unsatisfactory reliability, and when applied to the same group no two of them agree to any appreciable degree. We need very much to know whether there are such constellations of habits, tendencies, or dispositions which can be called extroversion and introversion. A measurement of the frequency of coincidence of the various partials which have been suggested for these two constellations, those items of the Freyd list, for example, will help to determine whether we are talking about such entities as extroversion and introversion and to decide just what they do include. The technique of Spearman (45) for testing for general, group, and specific factors may be applicable in this case, and may solve the riddle of personality traits in general. Having established the reality of such traits as extroversion and introversion, we are ready to look for simple objective tests and for some physiological basis for them.

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HUMOR AND THE LUDICROUS

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Since the article of Diserens (9) on laughter, in 1926, work on the subject has proceeded at about the usual rate. The classifications of laughter theories suggested at that time remain valid, no new type of theory having been developed. There is, however, an increasing tendency to stress social factors in the genesis and functioning of laughter. It will be noted that very few important contributions have been made.

Williamson (31) and Kimball Young (33) give historical accounts of the study of laughter and the ludicrous. Kimmins (22), in the first four chapters of his book, deals with the methods of studying laughter and historical theories of laughter from Hobbes to Freud and Bergson. Kimmins evaluates the theories and praises particularly the treatment of Hobbes, Spencer, and Bergson, for clarity and originality.

Dupreel (11) gives an historical review of theories of laughter. He distinguishes three types of theory: (1) Nonsociological theories of two classes: (a) the philosophical, treating of the nature of the object, and (b) the psychological, which is concerned with the character of the reaction. Hobbes, Descartes, Darwin, etc., are interested in this type. (2) Semi-sociological theories of laughter, of which the theories of Sully, Bergson, and Dumas are examples. (3) Sociological theories. Dupreel himself is chiefly interested in the sociological point of view, for the other points of view do not throw any light on the nature of laughter itself. Bergson and Sully tend in this direction, but their analyses are incomplete. The author insists on a complete sociological theory. He says: "Le probleme sociologique du rire, c'est tout le problem du rire." According to Dupreel, laughter has developed in a social setting and occurs in the relations of greeting and exclusion bound up with the life of the group. These two forms explain the ambivalent nature of complete laughter, viz., joy and malignity. Social life prepares in every one of us a kind of mechanism which may be set off by a myriad of possible incidents. Dupreel applies his theory to the consideration

of three problems: (1) The inequality of the sexes with respect to the comic, (2) the laughter of tickling, and (3) the nature of humor.

Hellyar (19) also stresses the ambivalent nature of laughter. The strongly paradoxical action, he says, depends on the "principle of double laughter." According to this principle, there are two laughs always necessary to laughter: comic and jolly laughter. Jolly laughter is the direct expression of jolly feeling; it is a spontaneous expression of delight, a direct response to a pleasurable feeling. It has no intellectual implications. Its only basis is the "interior feeling" of jollity which may result from a great variety of external causes. Jolly laughter covers all pleasure laughter; it occurs in situations that only the individual himself can know. Comic laughter is "laughter at" somebody or something. There is always some pleasure element in comic laughter. Both jolly and comic laughter are nearly always present in a specific situation. Laughter depends a great deal upon mood. When one is in a dull mood, comic incidents may go unnoticed. The spectator's state of mind must be one of mild and generalized pleasure. There must be a feeling of normal content. Self must not be involved in a laughter situation of this sort. The less one is disturbed, the greater the possibility of amused laughter. Jollity, therefore, is the best aid to the appearance of the laughter of amusement, for it provides the essential elements of that mental condition necessary to full comic laughter. A merry mood actually widens the comic horizon. Jollity seems to sharpen the comic appetite. Depression, on the other hand, saps the foundations of the comic mood and blunts comic perception. Taking this into consideration there should be a larger proportion of men of humor among the optimists and good natured men. Most theories explain the comic. Hellyar notes but does not explain the situation of a man laughing at something he finds comical. The two are complementary. Bergson, Hellyar says, omits the feelings of man completely. Comic laughter is largely a question of the nature of the individual. Laughter of amusement involves other factors besides, but its basis is also temperament. Both the emotional and perceptual elements of mind combine to make laughter an absolutely individual affair. A merry mind and a quick grasp of comedy are the chief arbiters in making or breaking laughter. Although jolly and comic laughter are very different, the man who laughs in the one key usually laughs equally well in the other. A merry mind is not necessarily in tune with the comic in nature but is a mind primed to respond, and response is easier when it does occur. If jollity can-

not get true comedy, it makes it. A sense of humor depends essentially on the presence of certain intellectual factors, such as the power of perceiving shades of meaning that pass unnoticed by a clumsy mind. A profound perception of the comic demands a thorough acquaintance with the world of things and events and an unusual and keen insight into the subtleties of life. The reason that our wisest men are not the greatest laughers, as one might suppose if the above were true, is that our men of wisdom are very often self-appointed and the wise-acres can hardly be expected to laugh and, partly, because the intellectual effort of hard thinking is not, in itself, an encouragement to laughter. But the real reason is the man himself, *i.e.*, his temperament. The wise man, when a laugher, is a splendid one.

Laughter, Hayworth (18) says, is a social, not a biological function. His theory is one of laughter as a means of communication and not a complete theory of the ludicrous. Before primitive man could talk laughter was a vocal signal to other members of the group that they might relax with safety. Facial expression as well as vocalization was, as it is today, a definite means of communication, and the smile was a visible symbol of the same meaning as, and derived from, the vocal signal. An aggressive, conquering attitude leads to laughter; an attitude of inferiority does not. The former fact is due to the presence of, and the latter to the absence of, a feeling of safety. We laugh more and heartier in a group than when alone because there are more individuals to whom we must convey our feeling of safety. When we hear someone else laugh, we are apt to laugh, for we are picking up the signal. This was very desirable in primitive times. We enjoy being laughed *with*, for that conveys a mutual realization of safety, but we resent being laughed *at*, for that implies the superiority of the one who is laughing. To sum up this theory, anything which disturbs the feeling of social safety or of individual safety has its corresponding effect on laughter. No one normally laughs unless he and his group are safe.

Joy, according to Hayworth (18), is the concomitant of laughter. Although laughter did not originally cause joy, according to his theory, Hayworth believes that the more we give ourselves to laughter the more fully other conditioned physiological changes will produce joy.

The writer of an editorial in the *Nation* (1), however, claims that the display of a sense of humor by laughing is not the act of a truly joyous man. He says that a sense of joy or a passionate interest in

some highly abstract and inexpressible thing is characteristic of the happy man, even though this sense of joy is almost always accompanied by solemnity. As an example of such a happy man, he gives the poet Wordsworth, whose works are by no means humorous, but which nevertheless emanate joy. The so-called humorous person, on the contrary, deems a sense of proportion, rather than an abstract passion, the most important thing in life.

J. J. Walsh (28) discusses the physiological side of laughter. The principal physical agent in laughter is the diaphragm. In ordinary respiration it moves up and down from two to three inches; in laughter from four to six inches. The result is that it has considerable effect on the important organs which have anatomical relations with it. These organs include the lungs, the heart, the liver, the pancreas, the spleen, the stomach, and the intestines.

Laughter also affects the ductless glands. Walsh says: "Laughter may be looked upon as an invention of nature to compensate for the diminution of organic friction and massage consequent upon man's erect position." Laughter relaxes mind and body and keeps us from considering ourselves too seriously. Walsh thinks that laughter is probably the most important of the diaphragmatic activities, all of which have a definite purpose for the safeguarding of health and the prevention of disease development.

In another article (4), Walsh stresses the connection between blood pressure and laughter. Man is the only animal who both laughs and suffers from blood pressure. Laughter, he claims, is a remedy for both high and low blood pressure. This seems significant both from a genetic point of view and also from a practical, for if the relation between blood pressure and laughter proves to be a really definite one in a large number of cases, undoubtedly it will be made use of in therapeutics.

Armstrong (6) also thinks laughter is physically as well as mentally beneficial.

A contribution intended to stimulate experimental studies is that of Carpenter (8). In 1922 he gave the superiority theory of Plato and Hobbes a new angle by calling laughter a glory in sanity. We laugh at recognizing our sanity, *i.e.*, our accuracy of judgment. For things to have a comic aspect they must be perceived (a) as false or deceptive, (b) abruptly or suddenly. Recently Carpenter (8) has offered the following corollaries: (1) Everything comic states or suggests something perceived as a falsehood. (2) Nothing conveying a falsehood is thereby comic so long as it deceives. (3) Nothing

conveying a falsehood is thereby comic unless it tends to deceive, or, in other words, unless an effort of judgment is needed to perceive the falsity. (4) Anything conveying a great enough falsehood that is suddenly perceived as such by an effort of judgment is comic (for a free and susceptible mind). (5) Of two things conveying falsehoods of equal degree the one requiring the greater effort of judgment to perceive the falsity is the more comic. This theory makes incongruity the basis of the comic. This dates back to Schopenhauer. But neither Schopenhauer or anyone else has explained why incongruity excites a pleasant emotion. It is the "effort of judgment" which is the crux of whatever is new in this theory. Carpenter says if it can be proved that the effort of judgment always occurs when the sense of the comic is aroused and that the sudden rejection of a sufficient falsehood by an effort of judgment will always arouse a sense of the comic in an open mind, then the agreeable quality of the emotion is associated in some way with the power of judgment. If it could be proved that the intensity of the emotion varies directly with the effort and also with the degree of discrepancy overcome by the effort, the final inference that the emotion is pleasure in the power of judgment would seem inescapable. This theory may be tested by determining whether an effort of judgment is thus related to the sense of the comic in the rejection of falsehoods. By "effort of judgment" Carpenter means a relatively strong exertion of the faculty that works abundantly and unceasingly in every conscious moment to interpret and correlate experiences. He does not mean obvious effort, necessarily. We can study this "effort of judgment" by studying at leisure the material on which it works. Carpenter has not studied this himself. He merely puts forth the idea for others to work on.

Myerson (26) notes that "It is one of the curious by-products of the comic spirit that it finds one of its most prolific themes in insanity. Everywhere on the stage and in the little comic by-plays of everyday speech and act which enliven the folk-life, the supposed conduct and speech of the insane man is a source of never failing merriment. This is in striking contrast with the attitude toward the so-called physical diseases. No comedian of the stage or of private life can raise a laugh with the comic portrayal of cancer or of heart disease, though the grotesque portrayal of the tragedy of mental diseases has given more than one clown a reputation. It is probable that this curious reaction toward the mentally sick arises in part, at least, from the fact that our sense of superiority is heightened in their presence,

while sympathy for them is, in the majority of people, neither fundamentally nor intensely aroused.

Kimmins (22) takes up the question of the development of laughter in children. "Laughter comes naturally when the neuromuscular mechanism is sufficiently developed for its production." Prior to this the baby smiles and makes sounds which later develop into laughter. In learning to walk the child appears to find delight in muscular mishaps. Incongruous sights and sounds of babyhood play a part in the genesis of laughter.

Washburn (29) made a study of fifteen infants. She found that smiling and laughing behavior vary considerably as growth proceeds, with more striking and consistent developmental differences in smiling than in laughing behavior patterns. Four personality types emerged from the differences in laughter behavior.

Enders (12), experimenting on children two, three, and four years old, found that sound and motion or a combination of both are the most effective elements in stimulating laughter in children of all these ages. Motion is the most effective with two-year-olds. The older children were interested in word play. All children laughed most frequently when with other children.

Kimmins (22) made a study of children between the ages of five and twelve. There was a marked increase in verbal humor. "Verbal humor seems to run along the lines of a logical sense of incongruity and of reasoning ability." Their visual humor seemed to depend upon their emotional development. "At the period of rapid growth from eleven and a half to the end of the thirteenth year, there is a decline in the sense of verbal humor." Visual humor is less affected. Unlike Enders, Kimmins found that there was correlation between a sense of humor and intelligence.

"In the earlier years there may be traced a fairly regular continuous growth of the appreciation of humorous situations which cause smiles or laughter, but in the period of rapid growth, from twelve to fourteen years, progressive development is difficult to trace; from fourteen to eighteen years, the springs of laughter . . . are comparatively easy to discover" (22).

Kambouropoulou (21) conducted an experiment on college girls to discover individual differences in humor. She found that there is a fair degree of consistency to types of humor. These types are primarily the personal and impersonal. Mental ability apparently bears no relation to these types. She concludes that experiments relating these types to temperament and character and determining

the relative influence of these and mental ability must be performed before the sense of humor can be further analyzed.

Wynn-Jones (32) tells of the following experiment: Two series, each of eighteen paragraphs, of various forms of wit were shown to children of both elementary and secondary schools, and to university graduates. The results of this experiment seem to show that locality plays a significant part in appreciation as compared with some general factor. If the point of a story is not manifest there is an active search for a clue.

A modification of McDougall's theory that laughter constitutes a protection to the organism against excessive suffering in sympathy with the maladjustment observed to inhere in the ludicrous situation has been suggested (16). Laughter situations to which it is difficult or impossible to apply the antidote hypothesis are cited. The most general formula for the modification seems to be (the author is not wholly clear on this point) that laughter represents a discharge of energy left unexpressed by the checking or failure of an object, of any mild conative tendency, provided the impulse to laugh be itself present in sufficient strength. Instances are cited and classified under the major impulses. Laughter is assigned a place in the scheme of impulses in the defense mechanism group, along with flight, the appeal or submission, and reduction or abolition of consciousness. The order in which these, together with the offense, group, anger, etc., are invoked is presented, with experimental evidence drawn from observations of an infant in typical fear situations.

We may now proceed to the categories of the ludicrous. It will be remembered that Freud contends that the ludicrous always represents some economy in the expenditure of mental energy. Wit, humor, and the comic comprise the three categories of the ludicrous. The pleasure of wit is due to the economy in the expenditure in inhibition, of the comic to the economy in the expenditure of thought, and of humor to the economy in the expenditure of feeling (9). In his recent discussions of humor, Freud adds that it is a triumph of narcissism and the pleasure principle. Humor is the assertion of the ego of its invulnerability. It involves the super-ego taking a parental attitude toward the ego as a child. When the ego is afraid of dangerous reality, the super-ego, when sufficiently inflated by its hypercathexis, can comfort it with humor, thus making the wounds inflicted by the outside world an occasion for pleasure (14). In cases where the humor is directed against the self, the ideal-ego becomes the parent which treats the ego as a child (15).

Humor is idealism tempered by sympathetic amusement, according to Robbins (3). Its basis is the perception of the discrepancy between what ought to be and what is, coupled with the belief that the difference, if realized, may be overcome.

Wilson (30) says that humor is a point of view. It comes from the sublimation of our unwanted impulses.

Real humor, according to Masson (25), must be true and must be put in such a form as to be startling.

Erskine (13) considers humor "the art of adapting one's self to another temperament." It is at its best, he maintains, when it is divorced from wit.

Attempts to arrive at a satisfactory definition of humor by means of a magazine contest reveal no new principle, to judge by the efforts quoted in the *Forum* (2). The following is typical of the definitions quoted: "Humor is the ability to obtain smiles, chuckles, chortles, grins, silent mirth, giggles, or laughs from our seeming calamities—the government of nations, relatives, taxes, religious doctrines, and modern male clothing."

Dunlap (10) gives the following classes of the comic:

1. The most primitive, includes bodily suffering and pain.
2. Suffering of a less serious nature, minor physical injuries, mental suffering.
3. Misfortunes and defects, physical and mental.
4. Suffering or misfortune accompanied by humiliation, sadness, degradation, on the part of the subject.
5. Moral defects, such as the beginning of vice in the young.
6. Horse-play or practical joking, premeditated or deliberate manufacture of laughable situations, manufacture of apparent accidents.
7. Degradation of imaginary characters in unreal situations, permitting of laughter on the part of those who are sympathetic in real situations.
8. The comic of embarrassment.
9. The mention or introduction of offensive matter, *e.g.*, the profane or the salacious.

The factors contributing to all of these types of the comic may be summed up under the general term "Inferiority." The degree of inferiority tolerated varies with the individual and the state of culture of his society, for the standard of reference by which inferiority is judged is usually one's self.

According to Hellyar (20), a man's notion of the comic is in-

tensely personal and spontaneous, being apt to occur when the mind is unoccupied. It is not necessarily pleasureable.

Wit is not specifically discussed in the articles under consideration.

The fact that age, sex, nationality, race, and breeding have an effect on one's sense of the ludicrous has been remarked by numerous authors. Kimmins (22) comments on the great tendency of children to join in the laughter of a group before the cause of merriment has been grasped. Between the ages of fourteen and sixteen years sex differences begin to appear. Extravagance of language in funny stories is less marked in girls than in boys. Boys tend to have a greater range and variety as regards humorous situations. There is much variation in sense of humor. At first, girls rank ahead.

In the case of negro children, the development of a sense of humor is generally retarded. In contrast to white children, at first, boys rank ahead.

Austin (7) takes up the question of sex differences. She puts forth a theory, clearly influenced by Freudian doctrines, to explain why it is that men are more apt to laugh at obscene jokes than are women. Man laughs because of the fact that nature has tied apron strings to him through the medium of his joy in procreation, and, after she has him securely bound, has let him in on the joke. Man laughs at what he fears as a defense mechanism. Woman, on the other hand, does not tend to laugh for three reasons: (1) because she undergoes the pain of creation and consequently looks at it in a serious light; (2) because she feels it is her duty to protect the youth so that their ideas of matrimony will be pure; (3) because it is part of her code, even though she realizes that the joke is on man, not to laugh at the failures of her lord and master. Mrs. Austin predicts that future generations of women will be able to appreciate the sex joke when they have been released from the drudgery of involuntary childbirth and can look upon such humor disinterestedly.

Walsh (28) thinks that the environment is responsible for the difference between the sexes. He predicts that the freedom now given to women will result in the diminishing, if not the end of, these differences.

Kimmins (22) says that the fundamental sense of humor differs little in England and America, but that the influence of professional humorists has an abnormal effect on the favorite subjects for laughter in America.

Kinosuke (23), by quoting two stories regarded as humorous by

the Japanese, endeavors to prove that Japanese and American humor are closely allied.

Kimmins (22) and Thomson (27) comment on the relation between laughter and tears. Kimmins says: "At times of great strain, laughter and tears come to each other's relief." Thomson points out that the stimuli for humor and laughter do not differ at the outset from those of sorrow and weeping. There is a tense moment of suspense which is followed by pain or pleasure, sorrow or joy. Painful results evoke sorrow; trivial results, joy and laughter, the latter accounting for the accompanying sense of superiority which is further heightened by surprise when that element is present. Wilson (30) notes that all humorous situations are disagreeable if taken seriously.

Laughter is a form of energy. Armstrong (6) says that it is a humanizing and socializing force. It settles more arguments than does logic. It restrains pioneers and radicals and detects imposters. It preserves the sane and normal. Mace (24) agrees with Armstrong that great possibilities lie in the power of laughter. Both predict that some day its energy will be controlled for social purposes, *i.e.*, the comic excesses of whole continents which culminate in world war will be checked. Laughter as a weapon will favor established institutions, according to Mace, since the fat and the prosperous can laugh best. Armstrong concludes by saying that laughter is one of mankind's most valuable functions which should be consciously cultivated and applied in promoting human efficiency and happiness.

Graves (17) says much the same of humor. He predicts that humor will be much better organized in the future, and he suggests many new possibilities for laughter which man's increasing mastery of science will bring in its train. Thomson (27) says that humor is a manifold means of social control which may make or mar the fortunes of the individual according to whether he is a participant or a victim. Wilson (30) declares that humor can do more than a League of Nations to keep peace in the world.

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PRECAUTIONS IN ANIMAL EXPERIMENTATION

The need for safeguarding experimental work upon animals is indicated by laws which from time to time are proposed in different parts of the country. An excerpt from a bill proposed last spring in the state of Illinois is given below:

"Any person who experiments on the body of a living animal for the purpose of ascertaining or demonstrating any fact in physiology or pathology, or for any other purpose, or any medical college, school or research laboratory, or the directors, trustees, managers, proprietors or employees thereof, who permits such experiments to be conducted in or upon the premises of such institution, or who keeps in captivity, any animal for the purpose of such experiments, or who refuses to permit any duly constituted officer or authority to make a full inspection of the premises of such institutions at any reasonable time, the time during which classes are held being deemed to be a reasonable time, shall be liable to a fine of five hundred dollars (\$500), or imprisonment in the county jail for not more than one year, or both." (Senate bill 221, Div. I, Sec. 50a.)

The above bill had a hearing on April 19, 1929, in the City Council Chamber, Chicago, at which its friends and foes expressed openly their views. Owing largely to the excellent work of the American Medical Association the bill was put to sleep during the hearing and never came to the floor of the legislature.

The incident shows the need for constant vigilance to protect animal experimentation from the propaganda of anti-vivisectionists and from the unenlightened actions of humane officers. The experience of England with anti-vivisection laws should be constantly before us.

What can the individual psychologist do to assist in the protection of animal experimentation?

(1) Experimental investigations with animals should be conducted according to the code of rules regarding animal experimentation¹ which was adopted in 1925 by the American Psychological

¹ The code is published in the *PSYCHOL. BULL.*, 1928, 25, 487-8. It has been recommended that this code be conspicuously posted in every laboratory where experiments with animals are conducted. In one laboratory all operators are required to sign the code.

Association. This code is now in force in all medical schools and research institutions in the U. S. A.

(2) Manuscripts, whether articles or abstracts, should contain no statements descriptive of cruel or inhumane treatment of laboratory animals. Editors are urged to reject manuscripts describing experiments which violate the code. In some cases positive statements should be made showing the steps taken to avoid needless suffering of animal subjects.

(3) In the classroom the psychologist can occasionally discuss the nature, requirements, and values of animal experimentation and the dangers from unenlightened legislation. This would help along the slow process of public education and possibly help to remove prejudices.

The American Psychological Association maintains a standing committee to look after the interests of psychologists in protecting animal experimentation. This committee aims to coöperate with agencies organized for the same purpose, to inform psychologists regarding the need and means of protecting animal experimentation, to distribute from time to time the code of rules regarding laboratory animals, and to act in the name of the American Psychological Association when a legislative emergency arises.

Suggestions, criticisms, and comments will be gladly received by members of the committee, which at present is composed of Professors Edward C. Tolman, Robert M. Yerkes, and the writer.

PAUL THOMAS YOUNG, Chairman,
Committee on Precautions in Animal Experimentation.

University of Illinois, December, 1929.

SPECIAL REVIEWS

EDWARD BRADFORD TITCHENER. *Systematic Psychology. Prolegomena*. Edited by H. P. Weld. New York: Macmillan, 1929. Pp. xi+278.

More than a century ago, when scientific activity was centered in France and scientific knowledge was beginning to get out of hand, the big French handbooks in the different sciences began to appear. For a while Germany translated them and then it took the initiative away from France. Germany was, and still is, the land of the all-inclusive, systematic, scientific treatise. Wundt, in promoting the new experimental psychology in Germany, fixed the habit of the encyclopedic handbook upon psychology. However, as the sciences became still more unwieldy, the possibility of a complete handbook by a single author was outgrown, as Wundt himself had discovered by the end of the century.

Titchener, as we all know, was the leader of the Wundtian tradition in America, in fact, the frame of reference in respect of which other American psychology could exhibit its heterodoxy. His greatest books are the four volumes of his *Experimental Psychology* (1901-05); they represent his effort to establish the scientific status of psychology. When he had finished them, he settled down to clarify his systematic position (1908-10).¹ After the publication of the *Text-Book* (1910) there were rumors among his associates that the (it was already called "the") big systematic treatise, after the Wundtian manner, was to be begun. However, Titchener delayed and wrote *A Beginner's Psychology* (1915), and then in 1917 actually began work on the *magnum opus*. The first volume was never finished. Some of Titchener's younger friends understood that he came presently to the conclusion that a systematic encyclopedic handbook was impracticable, and Titchener himself has told us of Wundt's similar decision of the same question.² On the other hand, Weld implies in his preface to the present volume that the larger project had not been completely abandoned; and it is possible that Titchener

¹ *Elementary Psychology of Feeling and Attention*, 1908; *Experimental Psychology of the Thought-Processes*, 1909; *Text-Book of Psychology*, 1910.

² *Amer. J. Psychol.*, 1921, 32, 173.

never made a formal decision, but allowed the matter to wait upon the completion of the volume of *Prolegomena*.

The present posthumous volume consists of an introduction and three chapters. A final chapter on Method was never written. The first writing was accomplished in 1917-19. There was some revision as late as 1923. Probably Titchener's health inhibited him later, and he died in the summer of 1927 before he could carry out his intention of writing the last chapter.

The Introduction to the *Prolegomena* was printed in 1921. Chapters I and II are entirely new matter. Most of Chapter III, the longest, was published in 1921-22, but it contains ten initial pages and eleven final pages that are new. Altogether, of the 250 pages of the text, 128 are new and 122 are reprinted. Yet Weld seems to the present reviewer to be right in thinking that the old material gains a new perspective in the present context. Before, this old material was little more than a secondary source for the views of other men and for facts about them. Now it snaps into relation with Titchener's general systematic views and becomes in part a key to the understanding of his method.

The Introduction to this book is called "Brentano and Wundt: Empirical and Experimental Psychology."³ This antithesis has been criticized,⁴ though of course Titchener did not mean that experiment is not empirical, but that empirical psychology, thus named, is not experimental.⁵ Philosophy tends nowadays to be empirical, and Brentano's method was the argumentative, empirical, nonexperimental method of the philosopher. Hence Titchener set the stage for his scientific psychology by exhibiting the contrast between Wundt and Brentano. He meant of course to bring the reader ultimately toward Wundt's position and away from Brentano's, but he left the choice between the two systems for the future. He ended the Introduction: "But Brentano also speaks of a 'science' of psychology. Which of the two authors is the right?"

Since this Introduction has been available to psychologists for nearly a decade, we can turn at once to the new matter of Chapter I. This chapter deals with the general "conception of science at large," and is the most original of the three chapters. There is in it a minimum of the citation and criticism of the views of other men,

³ First published in *Amer. J. Psychol.*, 1921, 32, 108-120.

⁴ L. Carmichael, *Amer. J. Psychol.*, 1926, 37, 521-527.

⁵ Titchener, *J. Gen. Psychol.*, 1928, 1, 176f.

which make up the bulk of the other two chapters. Instead, Titchener marshals vassals of his own choosing to pursue the argument up to the point where it becomes evident that a science is determined by its *point of view*.

He begins by saying that he is not seeking a logical definition of science. Philosophers have attempted formal definition, and they have, he thinks, all failed because they have not known science from the inside. What is needed rather is the scientific man's own conception of the science in which he works. This conception—we follow the thought of the chapter—can be got at in three ways: (1) One may consider the nature of the activity of actual scientific persons, thus seeing science in the concrete and the particular. This approach Titchener rejects, since individuals vary as to temperament and inclination, and no few men are entitled to represent science and to fix its nature by their own peculiarities. (2) One may, however, reject this approach to the conception of science, and still accept the method of delineating the “man of science” *qua* scientist, the abstracted, generalized scientist, the “perfect” scientist, as it were. This course Titchener adopts and follows throughout the first half of the chapter. (3) Besides this personal, individualized abstraction, there remains, however, science at large within civilization, “institutional science,” which transcends the activities of the scientists who contributed to it, and yet plainly has its larger modes of operation. To institutional science Titchener devotes the latter half of the chapter.

The abstract man of science is characterized by his possession of the “scientific temper,” and the *scientific temper* is described on its negative side by the adjectives “disinterested” and “impersonal.” Thus this man of science differs considerably from men of science in actuality, for “he does not chiefly value the things that men ordinarily care for; . . . he puts away reverence for antiquity, pleasure in the esteem of colleagues; . . . he lacks all desire for fame; he holds back his discoveries and shrinks from any sort of personal discussion.” A positive account of the scientific temper is, however, more difficult to give. The man of science, Titchener thinks, tends principally “to identify himself with his subject-matter, to lose himself in it, to become one with it.” He can find no better word for this state than “observance,” which for him “suggests something of the reverence for fact, sympathy with fact, loss of self in fact,” that characterizes the scientific temper. In all this Titchener is trying to stress observation as the very heart of all scientific activity,

and to turn his reader, if not toward Wundt, at least away from Brentano.

In the course of this argument Titchener emphasizes the point that the fact-minded man of science has no shred of interest in the *interpretation* or the meaning of his "observance," that he asks always the question "what?" but never "why?" or "how?" So again it is quite clear that the abstract man of science is very different from an actual living scientist. For this view Titchener found very little support from other writers. However, he did not expect support. In the first place, he must have believed that he had got hold of a crucial truth and that it was his duty to lead. In the second place, he presently returns to this point in a way that makes the position seem less extreme.

It is plain from what has been said that the *method of science is observation*. Observation is almost too simple to be defined. It is "sympathetic awareness," "direct acquaintance with." (It is not "knowledge about," for that implies interpretation.) Observation requires the scientific temper and also scientific training.

Thus *experiment* does not stand on its own feet as the method of science, although it has usually been so described. Experiment is the plan of observation. It involves repetition, isolation, and variation. Repetition, Titchener thinks, is necessary for completeness since not all of the whole is seen at once. Variation leads, as Mill showed, to generalized relationships. The reviewer does not think that Titchener has quite done his duty by experiment, for it is plain that repetition is necessary in experiment, not so much for the sake of gaining completeness piece-meal, as for the sake of ascertaining that all the essential relations have been observed, and that variation is the logical method for establishing functional relationships. The discussion of experiment could well have been placed under the section on "applied logic" instead of as a transition to it. In Titchener's exposition it looks as if scientific activity involved three terms, observation, experiment, and applied logic; whereas the middle term could properly have been subsumed under the last.

At any rate, Titchener passes on to the statement that an *applied logic* is essential to scientific activity. It is here in science that interpretation, the answer to the question "why?" in terms of the statement "because," enters in. Interpretation in this sense is necessary to system and to hypothesis and theory. It is "knowledge about," and it belongs in scientific activity as much as does the "acquaintance with" of observation. The two are coördinate, but Titchener insists

upon the dichotomy. He even suggests that a scientist might undertake only to observe with a logician to apply the logic to his observations! However, this division of labor is not what actually happens, and here Titchener presents a portrait of Darwin, as a dual personality, nicely balancing observation against logic. To the question of the validity of this sort of analysis we shall return later.

Titchener now turns to the broader field of *institutional science*. Its main task is to transform "acquaintance-with" into "knowledge-about," and the method at this more general level is called *description*. Description, like observation, does not deal with means, or end, or cause, or force, because these terms all imply the existence of a something that transcends the mere given of phenomena. Description as a method is analysis and synthesis. Analysis may be divisive, abstractive, or relational. Synthesis may yield classification, law of facts, or law of correlation. Analysis and synthesis together make up the one totality of description, which is, therefore, no more analytical than synthetic. The object of description is economy of thought without the distortion of observation. There is nothing new here, unless it is the indication that Titchener's psychology, when in his own words, cannot be the system about which *Gesalt* psychology is complaining.

Chapter II is called "The Definition of Psychology: Point of View." It begins with a discussion of the definition of the two other fundamental sciences, physics and biology, by point of view; it turns then to psychology at much greater length; it draws a conclusion; and it ends with an 'appendix' on biology and the relation of the three principal sciences. The method throughout is that of a critical review of sources. The method cannot be called an appeal to authority, because Titchener buttonholes the great, as it were, asks his questions of them, and then proceeds at leisure to demolish their answers. For all this formal parade of well known names, one ends with the conclusion that Titchener has no more respect for authority than did Socrates. It will not take us long to review this process.

For physics Titchener appeals to Lord Kelvin, citing the *Treatise* of Thompson and Tait, and he comes, after three pages, to the conclusion that "the definitions of the *Treatise* are, to be sure, obviously incorrect but they serve, nevertheless, to get the chapter started. Nothing further is required by a science whose self-sufficiency makes it indifferent to external criticism." For biology he picks out J. A. Thomson, and concludes after four pages that Thomson gives "a respectable working formula," which is spoiled by the fact that "the

author's logic appears to have been adapted to his controversial purposes."

For psychology Titchener begins, of course, with Wundt, and expounds his doctrine with a clarity and thoroughness of documentation that only Titchener's erudition could command in this field. However, he does not like Wundt's conclusion that physics deals with mediate experience and psychology with immediate experience, because it makes physics appear to be entirely artificial, and psychology much too simple. Moreover, he objects to Wundt's definition of biology as teleological. There are sixteen pages of Wundt. Next comes Avenarius with his definition of psychology as experience dependent upon the individual or the "System C." This is a clear and favorable exposition, for Titchener is going presently to approve of Avenarius' view. Then comes Mach. Titchener's admiration for Mach is known, but here he deserts him for Avenarius, approving of his views in so far as they are like Avenarius' and disapproving his definition of biology. Next Ward, in whom Titchener finds the germ of Avenarius' truth unduly obscured. After Ward, Külpe, who undertook to apply Avenarius' formula to psychology, and failed, Titchener thinks, by not understanding Avenarius. Then Ebbinghaus, whom Titchener finds muddled. Finally James, who came round at last to the definition by point of view, although his definition of psychology in the *Principles* is wrong. Perhaps these summary statements, for forty-six pages of careful research and exposition are not fair, and yet it does seem as if Titchener had called upon Baal and he had not answered—or at most had sent but a faint mist when Titchener had wanted a cloudburst. Well, the upshot of the whole matter is that Avenarius is right (as we knew all along, having read the story before) and that "psychology is the science of existential experience regarded as functionally or logically dependent upon the nervous system."

This second chapter ends with a discussion of biology, citing L. J. Henderson. Here Titchener comes to the conclusion that the environment plays the rôle of the independent term in the formula for biology that the nervous system plays in the formula for psychology. "Biology is the science of existential experience regarded as functionally or logically dependent upon the physical environment." He gives the three formulas for psychology, biology, and physics together (p. 142).

The third chapter of Titchener's *Prolegomena* is called "The Definition of Psychology: Subject-Matter." Its 124 pages constitute

about half of the entire book. Most of it, however, has been published before. In fact, Weld tells us that Titchener had the intention of letting this earlier publication stand without repetition, deleting from the book the long expository and critical sections on functional psychology and act psychology, and rewriting the chapter. However, the rewriting was never accomplished.

The purpose of this chapter is to examine the definition of psychology by subject matter. Titchener begins in newly published introductory sections with the clearing away of the current definition of psychology by illustrations, by the distinction of mind as "inner" and matter as "outer," and by the Cartesian dichotomy (that Bain used) of mind as nonextended and matter as extended. The sophisticated reader will not need to have the objections to these views explained to him. They were as unacceptable to Titchener as they would be to any modern epistemologist.

There then follows the long critique of American functional psychology, with Ladd (not Angell or Dewey) as the theme.⁶ After it comes the similar account of act psychology in Austria and Germany, with the views of Brentano, Stumpf, Lipps, Husserl, Messer, Witasek, and Stout making up the content.⁷ Both parts of the chapter are remarkably tolerant, sympathetic investigations of the views of others, conducted with Titchener's characteristic thoroughness. Titchener once told some of his friends that he spent an entire year "on Husserl," only to find out that there was "nothing in him." The section on the Germans has long been the standard secondary source in English for these authors. It is, however, a foregone conclusion that Titchener does not find for his present purposes, in either functional or act psychology, the definition by subject-matter for which he is looking.

In the last eleven pages of the chapter and of the book we get, however, what we have been waiting for all along, Titchener's own constructive thought. Here he returns to the problem of the three principal sciences and the possibility of defining them in terms of subject-matter. He is anything but dogmatic and doubts if his question is ready for final solution. Nevertheless, after a careful discussion of terms, he tentatively presents the following table:

⁶ Cf. *Amer. J. Psychol.*, 1921, 32, 519-542.

⁷ Cf. *ibid.*, 1922, 33, 43-83.

" SUBJECT-MATTER OF	FORMAL ⁸	MATERIAL ⁸
" Physics	Universal	Energetic
" Biology	Individuate	Behavioural
" Psychology	Systemic	Sensory "

The table might well be inverted, since there is in it an order of dependence from below upward. In psychology experience is regarded as dependent upon the biological nervous system; in biology experience is regarded as dependent upon the physical environment. The form of psychology is *systemic* because it is organized in respect of an organic system, actually the nervous system or its biological equivalent. The form of biology is *individuate* because here the unit is the individual in relation to its environment. The form of physics is, of course, *universal* because it exhibits no such limiting principle of organization. In material the subject-matter of psychology is *sensory* in so far as this adjective describes the nature of sensations, images, feelings, and all other conscious processes. The corresponding adjective for biology is *behavioral*, since behavior is the general term that covers the relation of the organism to its environment. In physics, where the subject matter is universal, the formal term is *energetic*, since it is in terms of energy that the organization of physical science is accomplished.

If there is any important criticism of Titchener's systematic method to be made, it will appear in the examination of his general procedure in the sort of discussion that this book represents. Three things stand out in the reviewer's mind:

(1) The first concerns his analytical method. It has been claimed—unfairly some say—that Titchener is an 'atomist' in his psychology. Be that as it may, he certainly is an 'atomist' in his argumentative procedure. Take, for example, his analysis of the method of science into observation and applied logic. Of course the scientific method has both an observational and a logical aspect, but Titchener does not call these two, 'aspects.' He cuts them clean apart, so that he can dwell upon observation alone. He suggests that observation and the application of logic might even be the activities of different men. He is emphatic in denying the propriety of applied

⁸ Through a printer's error, which Professor Weld has ruefully pointed out to the reviewer, the words "Formal" and "Material" have been interchanged in the headings to the table, although the use in the text is clear. The table is printed correctly here, but Weld has no occasion to be rueful. for several readers have as yet failed to find any other typographical errors in the entire book.

logic when he is discussing observation. When ultimately he performs his resynthesis of the two, he gets only an *Und-Verbindung*. Everyone who has introspected upon introspection knows that the applied logic is carried in the *Aufgabe* and gets down to the very roots of observation. Of course, Titchener could say that he was not talking about actual observation, but the ideal observation of the abstracted man of science. However, this abstraction is also too unreal to mean much for the scientist, and the reviewer at least cannot see what good is to come from an abstract observation so unlike any observation that ever occurs. The separation of the ideal man of science from the actual man of science is a similar analysis, and here we are brought into the opposite difficulty, in that Titchener does not keep the two entities distinct. In one place (pp. 30f.) he says of this ideal man of science, "if he is a Copernicus," "if he is a Newton," "if he is a Huxley," adding what he would do if he were one or another of these men. Copernicus and Newton and Huxley were free to approximate the abstract man of science as well as they could, but the abstract ideal is not free to model himself on a mere human pattern. It is hard to be sure in these passages that Titchener seriously intends an abstraction from actuality, and yet, if he does not, the validity of the separation of observation from applied logic fails.

In general, in Chapter I the reader feels as if he were transported to a strange world, where everything is new and everything distinctly different. The man of science, institutional science, observation, experiment, applied logic, description, and even analysis and synthesis stand out as separate independent entities, each to be discussed and described without relation to the others. After they have every one been separately encrusted with meanings, it is not possible for even Titchener to make them stick together into a single organic whole. The method has polemical advantages that make it possible to emphasize the importance of one factor without stressing the others, but it does not seem to yield a true picture of the whole.

(2) The second point has to do with a matter that is both a virtue and a defect of Titchener's writing. Titchener was, as he wanted to be, a man of science concerned with observation. Nevertheless in most of his writing he is engaged, not with matters of fact, but with matters of opinion. The essence of the scientific temper is humility, and 'observance' of the given without egoistic interpretation. Such a method is not, however, fitted for the realm of opinion. Titchener, the scientist, did not wish to construct ideas as philosophers do; he

wished to find them as scientists find the givens. Thus in the humility of 'observance' he seeks the views of others and laboriously lives with them and describes them; then with the egotism of logic he criticizes them and rejects them. This course is a misapplication of the method of science to the realm of opinion, and it results twice in this book in an elaborate and lengthy examination of the systematic views of the competent with the resultant discovery that all the work was to very little purpose as regards the main thesis. However, one good result is achieved. The reader finds an invaluable mine of information, even though he has to climb out of it again before he proceeds on his way.

(3) The last point raises the question as to whether there is any better method of dealing with systematic issues than the philosopher's. Titchener excommunicates the philosophers because they do not know science at first hand and thus cannot deal sympathetically with its problems. He wants the scientists to settle these matters for themselves. He finds, however, no great physicist or biologist, except perhaps Henderson, who satisfies him. He goes in vain to the greatest names in psychology for a definition of psychology, and turns away to Avenarius, the philosopher. It is not clear whether Titchener thinks that there is something intermediate between the *a priori* method of philosophy and the *a posteriori* method of science, which the scientist might use, or whether he merely believes that the scientist should be his own philosopher. Certainly all the general systematic matters about definition of science and of psychology, and thus the entire content of this book, are prior to science and psychology and are subject matter for philosophy. Titchener seems to the reviewer to have attempted to bring the scientific method to a philosophical problem and to have met with imperfect success. Only his bitter disappointment with philosophy in relation to psychology could have led him to do this, for he was everywhere else the champion of professional sophistication and expertness. It is true that he never admitted that these systematic problems for psychology are philosophy; but, if they are not, what are they?

EDWIN G. BORING

Harvard University

CHRISTINE LADD-FRANKLIN. *Colour and Colour Theories*. New York: Harcourt, Brace and Company, 1929. Pp. xv+287.

It will be a great satisfaction to psychologists to have available, in the compact form of a fine book, all of the writings of Dr. Ladd-

Franklin on the subject which she has made so definitely her own. The problem of color vision has fascinated keen scientific minds through the centuries: Leonardo, Newton, Young, Helmholtz, Hering, G. E. Müller, and others; but it is not too much to say that the present work, by its firm grasp of the numerous relevant facts, and by its incisive logic, deserves a place even in so distinguished a company. The book brings together, with notes and comments by the author, papers published from 1892 on to 1926. It is chiefly devoted to aggressive but good-tempered argumentation in support of the author's own theory of color vision and against rival theories. Not only the classical theories of Young and Helmholtz and of Hering, but many more recent attempts, are subjected to destructive criticism on the basis of the facts. Though the facts are used as weapons, they are present in such abundance and precision as to make the book a mine for those who are more interested in fact than in theory.

As is well known, Dr. Ladd-Franklin maintains that Helmholtz in his tricolor theory neglected the facts that Hering stressed, and that Hering in his theory of antagonistic colors disregarded the quantitative data established in the Helmholtz laboratory, while neither of them did justice to the psychologically unitary character of all the colors that demand recognition as elements: yellow and blue, true red and green, black and white. She was able, by bringing in photochemical considerations, to indicate a way in which all these facts could be covered by one theory. Moreover, her theory was easily stated in a developmental form. If one value of theory is to hold together a mass of facts, the Ladd-Franklin theory of color vision is remarkable for its success in integrating the facts of direct color impressions, of color mixing, and of rod and cone vision, along with something of photochemistry and of biological evolution.

Whether the theory has also a heuristic value is another question which has sometimes been answered in the negative. The author herself found it of value, years ago, in predicting foveal blindness in total color blindness and in normal twilight vision. Its possible use in suggesting research into the photochemistry of vision has perhaps not been tried out as yet. On the developmental side, it suggests that primitive types of color vision might be found among animals, conforming either to red-green blindness or to total color blindness; and this prediction has already been verified to a certain extent.

Quite recently Hecht has been stimulated in part by the Ladd-Franklin theory to reëxamine the matter of binocular color mixture,

and has found conditions under which a binocular combination of red and green gives the sensation of yellow. This result seems at first thought to invalidate the Ladd-Franklin conception of yellow as the result of chemical combination, probably within the retina, of the red and green cleavage products. But there are so many facts yet to be brought into line with the conception of a purely cortical origin of yellow that we can hardly regard the Ladd-Franklin theory as shattered by this one blow. What does seem probable is that renewed activity is now to be expected in the study of color vision, and that the theory is destined to play its part as a heuristic principle.

One advantage of the Ladd-Franklin theory over that of Hering appears in the treatment of black. Both theories—and the Young-Helmholtz theory as well, in order to be complete—have to assume a cortical response as occurring when and where no stimulation is coming in from the retina. The conception of antagonistic colors requires that black shall correspond to actual stimulation coming in from the retina, and so is forced to postulate a medium gray as its background or cortical color—which postulate forces it into several improbable supplementary hypotheses. The Ladd-Franklin theory is free to select black as the cortical color corresponding to no stimulation from the retina; and this assumption is shown to work out quite neatly.

Mention should be made of the color charts, which are not only beautifully executed, but very carefully managed to present the colors required; and the glossary should not be overlooked, with its definitions and important suggestions of standardized nomenclature.

R. S. WOODWORTH

Columbia University

MURPHY, GARDNER. *An Historical Introduction to Modern Psychology*. New York: Harcourt, Brace, 1929. Pp. xvii+470.

At last, experimental psychologists have a history of psychology which they will undoubtedly enjoy reading. Previous histories have dealt for the most part with the philosophy of mind. Dr. Murphy's desire in writing the book was to give a perspective for contemporary scientific psychology, and his mental set is therefore an experimental one. It is true that the first part deals principally with the work of the seventeenth and eighteenth centuries, but even here he centers upon scientific work, such as that of Cuvier and Bell, which has a bearing upon modern problems of psychology, biology, and

physiology. These chapters, however, are merely inserted to supply the knowledge necessary to an understanding of what is to come. The history is virtually sketched between the frame of the nineteenth and twentieth centuries, and thus begins at the time when progress in biology made possible a relatively sharp division between philosophy and psychology. The events are not recorded in strictly chronological order, nor do the chapter headings seem to fit into a logical scheme. Yet the history develops smoothly and clearly, and there is something refreshing in the freedom from any rigid arrangement.

The second part of the book, which follows the preexperimental period, covers the ground from Weber to Wundt. Much space, of course, is given to Weber, Müller, Fechner, Helmholtz, and Hering. There is also a lengthy description of the Associationists, the Evolutionists, and the French psychiatrists. Wundt's contributions, and those of his pupils, especially Cattell, are developed in detail.

Part Three deals with various topics of experimentation, except for the chapter on William James, which is introduced dramatically between a chapter on memory and one on functional and structural psychology. Then follow chapters on the thought processes, acquisition of skill, behaviorism, child, social, and religious psychology, psychoanalysis, instincts, intelligence, personality, and physiological psychology. These topical chapters do not always contain what one would expect. One finds under the chapter on personality, for example, descriptions of the classical work on the emotions. For the most part, however, Dr. Murphy has used excellent discrimination in the selection of his material, and these chapters might well serve as models for graduate seminary papers.

At the end of the book, by way of good measure, we are given two chapters on contemporary German psychology by Heinrich Klüver. It is rather illuminating to compare the last pages of Dr. Murphy's contribution with the opening pages by Dr. Klüver. Dr. Murphy extols the development of quantitative method, and states that "it is, perhaps, not entirely fanciful to suggest that as our knowledge and our language becomes more precise, the answer to the question 'how much does it hurt' may be $42xy^3\cos A$." Dr. Klüver writes, "Viewed historically, it seems, at least so far as German psychology is concerned, that the chief trend is towards a qualitative psychology. . . ." Dr. Klüver's last chapter on psychology as a "cultural science" will cause the experimentalist to thank the fates for his laboratory.

One is not always able to agree with Dr. Murphy's interpretations, nor with the emphasis he has placed upon certain facts. For example, to say about Ebbinghaus' quantitative treatment of learning and forgetting that "this was probably the greatest triumph of original genius in experimental psychology since Weber," appears to the reviewer a bit strong. However, it is easy to criticize a history. Dr. Murphy has written his book in a style that makes its reading a pleasure. He seems for the most part enthusiastic over his subject, and the reader is infused with his spirit. He has avoided details, which is a wise procedure in a history of this nature. But, of course, it means that the reader must already have a background of psychology, in order clearly to understand many of the descriptions, especially those concerned with experimentation. The book will probably not have a popular appeal, but it will be appreciated by those persons who have more than a superficial interest in psychology. It will prove invaluable to the advanced student, and will be read with much profit by the instructor.

H. S. LANGFELD

Princeton University

GOLDSTEIN, K., *Psychologische Methoden zur Untersuchung der Hautsinne*. SCHAEFER, K. L., *Psychologische Akustik. Handbuch der biologischen Arbeitsmethoden*, Abt. VI, Methoden der experimentellen Psychologie, Teil A, Heft 3 (Reine Psychologie). Berlin: Urban and Schwarzenberg, 1922. Pp. 149.

This monograph contains two thoroughgoing reviews, one of them being concerned with the experimental methods, apparatus, and data pertaining to the cutaneous sensory modes and the second giving a similar comprehensive summary of investigations in the auditory field. The author of the first article, K. Goldstein (Frankfort), acknowledges an emphasis on the function of the tactual senses in relation to the whole of the mental life. The method of research in this field must be grounded on expressive movements manifested either by the involuntary reflexes or by voluntary speech. He shows the influence of the principles underlying the psychology of Gestalt in stating that the analysis of cutaneous qualities is misleading because of the proved reciprocal effects among sensations of various orders and because of the fact that a single quality deprived of its simultaneous or even its successive associates can not be regarded as a component part of the total experience. Cutaneous localizations

are affected by visualization of the *locus*. Even specific conditions, such as observation with or without closed eyes, modify the results.

The author goes on to characterize and criticize the various classical procedures, like the method of limits, that involve threshold values, the methods that apply to supraliminal values of intensity, and the method of paired comparisons. He has collected the more important data concerning bodily localization and sensitivity to various orders of stimuli in different parts of the body, and presents detailed tables and diagrams to illustrate these facts.

A separate section develops and systematizes the results of experimental work on the cutaneous qualities of the several orders. Among the more significant treatments rank the data concerning the recently investigated "sensations of vibration." These experiences are apparently not referable only to the periostium, as was once believed, but are reported also in connection with pressure sensations of cutaneous origin. The author believes that they are not new qualitative distinctions but are due to the peculiar manner in which these sensations of pressure are aroused. For clinical purposes, however, the bodily protrusions still lend themselves best to diagnostic analysis. Clinical procedures are further discussed in detail and pathological results are noted.

Thereupon follow drawings and descriptions of standard pieces of apparatus in this field of research, together with critical notations of their peculiar limitations and their special adaptations. A carefully selected bibliography of 134 titles is appended.

The article on psychological acoustics written by K. L. Schaefer (Berlin) is written on an equally high level and is comprehensive in scope. It is especially valuable in its critical discussion of experimental techniques and of methods used in the standardization of apparatus. It begins with a presentation of the main facts concerning the demonstration and the observation of very low tones followed by a similar section on the tones near the upper limit of hearing. There an unusually complete description of the techniques involved in the standardization of the Galton whistle is given—the reviewer has nowhere seen so comprehensive a survey of these important procedures. The next section deals with the lower limen for intensity. Tones of very short duration and the various phenomena surrounding interrupted tones are then rather adequately treated without uncovering much of intricate mathematics and physics. Pitch discrimination is also done on a moderately comprehensive scale. Much of the American work in all of these fields is apparently either unknown or

disregarded. But there is also a really negligent disregard of the important psychological principle that has appeared in our work only in the last decade that pitch discriminations are functionally related to degrees of intensity, so that actually we can no longer speak of "tonalgaps" or "tonal islands."¹ The next section reviews the procedures and results in the field of intensive discrimination of sound. Then comes an account of some of the work on the judgment of intervals. Again while there is much helpful material concerning the classical procedures and the outstanding results, with a critical appraisal of the several methods of attack, there is a dearth of discussion of the experimental work on consonance and dissonance which has come within the chronological scope of the article. The last section summarizes much of the work on sound localization in terms of angular displacement from the ears, and the perception of the distance of sounds. For the size of the entire article, this section is exceptionally well done. It is a subject which brings in new facts each year and some of the classical experiments consequently require reinterpretation. In a field where the accumulated facts demand a separate monograph several times as extensive as the whole of this one, it is easy to point out that there are many investigations, which in the reviewer's mind loom large and which have been omitted in the discussion. The bibliography is not collected at the end but is carried concurrently as footnotes.

As an easily accessible monograph on these two sensory modalities which discusses methodology and apparatus on a moderately high level of constructive and destructive criticism the laboratory technician would do well to welcome this monograph into his library. For the laboratory assistant, who has a fluent command of German, the reviewer knows no better helpmate in the field. The monograph forms one of a complete series on the various subdivisions of experimental and other branches of psychology.

CHRISTIAN A. RUCKMICK

University of Iowa

BAGBY, ENGLISH. *The Psychology of Personality*, an Analysis of Common Emotional Disorders. N. Y.: Holt, 1928. Pp. 236.

The *Psychology of Personality* is limited to the "study of persistent emotional tendencies," with the emphasis placed upon the

¹ Ruckmick, C. A., Auditory Sensations and Related Phenomena. *PSYCHOL. BULL.*, 1927, 24, 84 ff.

"purely descriptive studies" "of minor disturbances of thinking and conduct." A conservative interpretation of the phenomena studied led to the development of certain "activity levels" and a new theory of adjustment termed the "tension-reduction principle."

"Personality traits" or "emotional complexes" are the persistent characteristics of an individual. These must be developed, or "reduced," or new complexes "conditioned" to replace the old.

Unfortunately the case used to illustrate the introductory explanation of the point of view is a "sex repression" case. However, the author gives a reasonable discussion of the numerous emotional phenomena. Many of these are significant from daily life, for example, "worry."

The concept of "tension" is developed from the relation of "changing internal conditions" and the expenditure of energy. This "condition" is important because it is capable of changing the "intensity of reactions."

The "basal activity level" of the individual is the hereditary background of internal function, while the emotions tend to determine the internal change and so the actual level. The basic problem of adjustment is the necessity of altering the "tension" by the "conditioning" of more adequate "emotions." The reduction of "emotional tension" becomes the main object of treatment in all of the "disturbances" of reaction. The usual psychiatric and psychological treatment of abnormal cases is presented in the form of illustrative material.

A statement of general principles of treatment with its undue emphasis upon "inferior" adjustments of "fear arousing experiences" leaves much to be desired.

In all the author has presented a point of view which perhaps needed fewer pages for statement, but many more for adequate application.

R. A. BROTEMARKLE

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CRAWFORD, A. B. *Incentives to Study*, a Survey of Student Opinion. New Haven: Yale Univ. Press, 1928. Pp. 194.

An analysis of tendencies and motives among College students is made on the basis of an investigation carried out with the assistance of a large number of undergraduates at Yale. It indicates the factors influencing the attitude of college-level student toward the

materials, problems and processes of their education. It aims to understand the motivation of the student's "essential job of study."

The Survey conducted in 1926 was formulated by students and endorsed by the Undergraduate Student Councils. During the two years required for analysis, the interest of the student body crystallized in a separate Report which is presented in the appendix.

Evidence is presented to establish the authenticity of the data secured from approximately half of the undergraduate student body. The author insists that "small but consistent differences properly merit recognition" and "in light of the numerous factors operating may be said to have great significance."

Initial ability or general intelligence, based upon Intelligence Test Standing, is held to be of greatest importance.

Economic factors were investigated in terms of family, home, annual expenses and degree of self-support. Academic success is held to be inversely related to financial "advantage."

Family backgrounds were investigated in terms of "occupation of parents" and "parent's education." "The non-college family groups" present better scholastic risks; while the individuals from "smaller college families" do not do as well as those from "large families," "probably because of the spur of competition."

Occupational Purpose enhances the scholastic application with the greatest effect on the part of the professions as over against the prospective business men.

A coördination of the various findings places the incentives in order as follows from greatest to least: Potential Ability, Life Purpose, Professional Career, Economic Status, Parents' Occupations, Student's Own Purpose in Going to College.

Student Activities maintain a definite influence on "purpose." The investigation was supported largely by the leaders of student activities.

The Curricular Requirements and Electives indicate that there is less value educationally in the former, especially if the requirements are not in the student's major field of study. Intellectual breadth is to be attained through capitalization of the value of purpose.

The author concludes with a statement of the necessity of correcting the curriculum and developing its function in terms of the student's "purpose," and in harmony with his attitudes and interests. Orientation and guidance is required to develop the intellectual purpose of the student in his earlier years, and direct him in his later years of study.

The appendix contains a bibliography of 156 books or articles, the Questionnaire and Time Chart Forms, with procedure, Superiority data and the Special Student Report.

It is not enough to say that the analysis of the survey has sustained the opinions of educators, or in a comprehensive way established the results of former studies. The study points to advanced methods of Student Personnel investigation, with improved educational instruction and guidance. The greatest import of the survey will remain the reasonableness of the "student opinion" and the advisability of administrative use of the same.

R. A. BROTEMARKLE

University of Pennsylvania

BOOKS RECEIVED

H. MELTZER and EDWIN M. BAILOR, *Developed Lessons in Psychology Including Objective Tests with Norms*. N. Y.: Harcourt, Brace, 1929. Pp. x+199.

CARL MURCHISON (Editor), *The Psychological Register*. Worcester: Clark Univ. Press, 1929. Pp. iv+580.

FOWLER D. BROOKS, *The Psychology of Adolescence*. Boston: Houghton Mifflin, 1929. Pp. xxiii+652.

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LEONARD T. TROLAND, *The Principles of Psychophysiology*. A Survey of Modern Scientific Psychology. Vol. 1, The Problems of Psychology; and Perception. N. Y.: Van Nostrand, 1929. Pp. xix+429.

HSIAO HUNG HSIAO, *An Experimental Study of the Rat's "Insight" within a Spatial Complex*. Berkeley: Univ. of Calif. Pub. in Psychol., 4, No. 4, 57-70, 1929.

ROBERT C. TRYON, *The Genetics of Learning Ability in Rats*. Berkeley: Univ. of Calif. Pub. in Psychol., 4, No. 5, 71-89, 1929.

HAROLD J. SAVAGE, and others, *American College Athletics*. Bull. 23, Carnegie Foundation. N. Y.: 1929. Pp. xxii+383.

MAX F. MEYER, *The Musician's Arithmetic*. Drill problems for an introduction to the scientific study of musical composition. Univ. of Missouri Stud., 1929, 4, No. 1. Pp. 149.

HENRI PIÉRON, *Principles of Experimental Psychology*. (Trans. by J. B. Miner.) N. Y.: Harcourt, Brace, 1929. Pp. viii+190.

JUNE E. DOWNEY, *Creative Imagination*. Studies in the Psychology of Literature. N. Y.: Harcourt, Brace, 1929. Pp. viii+230.

HAROLD JEFFREYS, *The Future of the Earth*. N. Y.: Norton, 1929. Pp. 79.

H. STAFFORD HATFIELD, *The Conquest of Thought by Invention*. N. Y.: Norton, 1929. Pp. 80.

P. SARGANT FLORENCE, *Sociology and Sin*. N. Y.: Norton, 1929. Pp. 98.

CARL MURCHISON (Editor), *The Foundations of Experimental Psychology*. Worcester: Clark Univ. Press, 1929. Pp. x+907.

THE PSYCHOLOGICAL BULLETIN

THE RAT

A Bibliography

L. E. DRAKE and W. T. HERON, with the assistance of MILDRED BURLINGAME,
L. E. BALLACHEY, and G. PAULSEN
University of Minnesota

This bibliography was started with the idea of annotating the references but it was found that space would not allow this unless only a small minority of references was included. We have taken the alternative of including the majority of references and compiling an analytical index.

The bibliography covers the years 1924 to 1929 inclusive, except that the psychological references are complete for all years. Time and space has not allowed us to cover foreign journals. It is too much to hope that some errors have not occurred but we believe that they are not many. We trust that the bibliography will be of use to other scientists as well as psychologists.

The following journals have been used in the compilation of this bibliography. In the majority of cases the files are complete to the end of 1929.

American Journal of Anatomy	Amer. J. Anat.
American Journal Diseases of Children	Amer. J. Dis. Child.
American Journal of Hygiene	Amer. J. Hyg.
American Journal of Medical Sciences	Amer. J. Med. Sci.
American Journal of Pathology	Amer. J. Pathol.
American Journal of Psychology	Amer. J. Psychol.
American Journal of Tropical Medicine	Amer. J. Trop. Med.
American Medicine	Amer. Med.
American Naturalist	Amer. Nat.

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| Anatomical Record | Anat. Record |
| Annals and Magazine of Natural History | Ann. and Mag. Nat. Hist. |
| Archives of Dermatology and Syphilology | Archiv. Derm. and Syph. |
| Archives of Internal Medicine | Archiv. Int. Med. |
| Archives of Neurology and Psychiatry | Archiv. Neur. and Psych. |
| Archives of Ophthalmology | Archiv. Ophth. |
| Archives of Pathology and Laboratory Medicine | Archiv. Pathol. and Lab. Med. |
| Behavior Monographs | Behav. Monog. |
| The Biochemical Journal | Biochem. J. |
| Biological Bulletin | Biol. Bull. |
| Biometrika | Biometrika |
| Brain | Brain |
| British Journal Experimental Biology | Brit. J. Exp. Biol. |
| British Journal of Experimental Pathology | Brit. J. Exp. Pathol. |
| British Journal of Psychology | Brit. J. Psychol. |
| British Journal of Psychology — Monograph Supplement | Brit. J. Psychol. Monog. Suppl. |
| The British Medical Journal | Brit. Med. J. |
| Carnegie Institution Contributions to Embryology | Carnegie Inst. Contr. Embry. |
| Columbia Archives of Psychology | Col. Archiv. Psychol. |
| Comparative Psychology Monographs | Comp. Psychol. Monog. |
| Endocrinology | Endo. |
| Genetic Psychology Monographs | Genet. Psychol. Monog. |
| Harvard Psychological Studies | Harv. Psychol. Stud. |
| Indian Journal of Medical Research | Indian J. Med. Res. |
| Journal of Agriculture Research | J. Agri. Res. |
| Journal American Medical Association | J. Amer. Med. Assn. |
| Journal of Anatomy | J. Anat. |
| Journal of Animal Behavior | J. An. Behav. |
| Journal of Bacteriology | J. Bact. |
| Journal of Biological Chemistry | J. Biol. Chem. |
| Journal of Cancer Research | J. Cancer Res. |
| Journal of Comparative Psychology | J. Comp. Psychol. |
| Journal of Experimental Medicine | J. Exp. Med. |
| Journal of Experimental Psychology | J. Exp. Psychol. |
| Journal of Experimental Zoology | J. Exp. Zool. |
| Journal of General Physiology | J. Gen. Physiol. |
| Journal of Genetics | J. Genet. |
| Journal of Hygiene | J. Hyg. |
| Journal of Immunology | J. Immun. |
| Journal of Infectious Diseases | J. Infect. Dis. |
| Journal of Laboratory and Clinical Medicine | J. Lab. and Clin. Med. |
| Journal of Medical Research | J. Med. Res. |
| Journal of Metabolic Research | J. Met. Res. |
| Journal of Morphology | J. Morph. |
| Journal of Nervous and Mental Disease | J. Nerv. and Ment. Dis. |

Journal of Pathology and Bacteriology	J. Pathol. and Bact.
Journal of Pharmacology and Experimental Therapeutics	J. Pharm. and Exp. Thera.
Journal of Physiology	J. Physiol.
Lancet	Lancet
Pedagogical Seminary and Journal of Genetic Psychology	J. Genet. Psychol.
Psychobiology	Psychobiol.
Psychological Bulletin	Psychol. Bull.
Psychological Review	Psychol. Rev.
Psychological Review Monographs	Psychol. Monog.
Quarterly Journal of Experimental Physiology	Quart. J. Exp. Physiol.
University of California Publications in Psychology	Univ. Calif. Publ. Psychol.
Australian Journal Experimental Biology and Medical Science	Australian J. Exp. Biol. & Med. Sci.

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NOTES AND NEWS

MARY WHITON CALKINS, Professor of Philosophy and Psychology at Wellesley College, died on February 27th in her sixty-seventh year. Professor Calkins had been President of both the American Psychological Association and the American Philosophical Association. She had been associated with Wellesley College since 1888, where she established one of the first laboratories of psychology in this country.

PROFESSOR EDWIN G. BORING, of Harvard University, has been elected Vice-President of Section I (Psychology) of the American Association for the Advancement of Science.

By a gift of the late Edgar Pierce (Ph.D., Harvard, 1895) a Lectureship in honor of William James has been established at Harvard University. Appointments will be made to this Lectureship at least biennially, the appointees being in residence at the University and participating in the regular instruction. Professor John Dewey will be the first lecturer on this foundation. During the spring term beginning February, 1931, he will deliver a series of ten public addresses on a topic not yet announced. The subjects covered in the William James Lectures are not limited in any way, beyond the general condition that they must fall within the field of philosophy and psychology.

THE Sixth International Conference on Psychotechnics will be held at Barcelona, Spain, April 23-27, 1930. The Secretary is Dr. E. Mira, Barcelona.

THE International Congress for Mental Hygiene, to be held in New York, May 5-10, 1930, is to be attended by many foreign delegates. Some of these are available for lecture engagements. For information apply to Dr. George S. Stevenson, 370 Seventh Avenue, New York.

ANNOUNCEMENT of appointments of National Research Fellowships in the Biological Sciences include the following in psychology: reappointments, G. LaVerne Freeman and Donald K. Adams; new appointments, O. D. Anderson, L. W. Gellermann, Dorothy K. Postle, T. C. Schneirla, J. H. Tiffin and John S. Yerakis.

APPROPRIATIONS of the National Research Council's Committee on Grants-in-Aid include the following: Thomas R. Garth for a study of the incidence of color blindness among Indians; Roland O. Travis for an experimental investigation of the speed of reflex and voluntary eye movements and to the Division of Anthropology and Psychology for a preliminary study of the development of negro children.

THE PSYCHOLOGICAL BULLETIN

VISUAL SENSATIONS: A REVIEW OF RECENT LITERATURE

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and Hospital*

Two years ago, Sloan (119) reviewed the literature on visual sensations for 1926 and 1927. The publications of the succeeding eighteen months are considered in the present paper.¹ Of these, some are primarily up-to-date presentations of the general knowledge about the fundamental facts of visual sensation. Troland (129) has shown the importance of these data as the best established and organized that we possess in the field of psychology, and has emphasized the fact that the physical, physiological, and psychological conditions of the arousal of specific visual sensations are all deserving of investigation. Elsewhere (130), he has brought together conclusions concerning the fundamental facts in this field as established by various experiments, analyzing and systematically relating available data. A similar presentation of the basic facts has been written by Parsons (103). In a more exhaustive discussion, Tschermak (131) has brought together the facts concerning many particular aspects of sensations for which light is the adequate stimulus, giving a comprehensive digest of this material.

Piéron (106) has drawn upon data in the field of visual sensation in his consideration of fundamental laws of sensation in general. He concludes that the multiplicity of variables involved in sensory discrimination makes impossible the formulation of a general law of

¹ Only those articles published in English, French, or German have been listed. Some are included which appeared in the latter half of 1929, but it has been impossible to cover that period with any thoroughness.

relationship between stimulus and sensation of the Weber-Fechner type. He remarks that the Weber-Fechner law breaks down outside the middle range of intensities, and that it neglects to take into account the variations in response with different durations of stimulation. Reporting an experiment in which he found that the limiting duration of response to a particular intensity of illumination, as measured by the critical frequency of flicker, varies with a change in the relationship of the phase of stimulation to the phase of interruption, and increases with increase in the magnitude of interruption, Piéron (108) again indicates the insufficiency of our present formulations of facts. In his investigation of Talbot's law, however, he (107) finds that this particular law is valid not only for stimuli of relatively long duration where a certain sensation level is reached, but also for very brief stimuli, even for those below the threshold of fusion. Haschek (55), on the other hand, seems to consider it an established fact that Talbot's law fails when exposures are very short: 0.001 to 0.0001 second. He regards it as a merit of his photochemical, tri-color theory of vision that it explains this fact as well as color changes in chromatic adaptation, decrease in saturation with increase in intensity, and the Purkinje phenomenon.

Spencer (122, 123, 124) has investigated Heymans' law of inhibition and finds that his experimental results confirm Heymans' hypothesis. With binocular vision, the liminal value of a second light increases in direct proportion to the increase in intensity of the inhibiting light. When the test light is viewed by one eye, and the inhibiting light by the other, a similar effect of the inhibiting light was noted. This indicates that the inhibitory effect must be explained in terms of a central connection. The fact that the absolute threshold was found to be relatively high when the observer felt most "fresh," and low when he was more fatigued, indicates that "freshness," high organization of activities, and high coefficient of inhibition are found together; while fatigue is associated with disintegration and a low degree of inhibition.

Many other investigators have been concerned with phenomena more specifically limited to the field of vision. Cobb and Moss (14) have investigated the effect upon the visibility of stimuli of the four interacting factors: visual angle subtended, brightness level, brightness contrast, and duration of exposure. Curves of threshold visibility show variation of the liminal visual angle with per cent contrast, with three different illuminations of the ground, and for three different durations of exposure. A further study (12) indicates that

when one of these conditions fails to favor visibility, it can be compensated for by more favorable conditions in respect to some of the other variables. It appears that the optimal brightness for large, low-contrast objects is at a higher level than for small, high-contrast objects.

The decrease in visibility caused by the glare of a 100-watt frosted tungsten lamp placed in the vertical meridian above the fixation point was also tested by Cobb and Moss (15). The effect was scarcely appreciable by this test when the glare source was more than 10 degrees above the fixation point even when discomfort was experienced. Benford (5) has suggested that the muscular strain associated with the effort to accommodate for one point when a glare source tends to distract attention to another point is probably an important factor in the fatigue associated with glare. Stiles (126), investigating the effect of glare on the brightness difference threshold, found that the Fechner fraction suddenly increases when a background brightness of two candles per square foot is exceeded. Danjon (19) has discussed the part played by brightness and contrast in determining acuity. He finds that an empirical formula represents the variation in visual acuity as a function of brightness contrast between the test object and the ground, and of the least perceptible contrast at that brightness level. Variations from the relationship at higher illuminations are ascribed to glare effects and differences in pupil size.

Ferree and Rand (32) have shown how increase in intensity of illumination effects an increase in speed of discrimination. Details subtending visual angles of 1, 2, 3, 4, and 5 minutes were discriminated under illuminations varying from 5 to 100 foot-candles. It was noted that at 5 foot-candles, a black broken circle on a white ground was discriminated more quickly than a white broken circle on a black ground, although the reverse held at higher intensities. These facts are explained in terms of irradiation of the white ground and the masking effect of a white after-image on black. Varying effects of specular reflection under different conditions were also noted. Ferree and Rand (33) have also investigated the advantages to be derived from increasing the intensity of illumination, by determining the speed of discrimination at different illuminations of details of different sizes, and with different percentages of brightness contrast between object and background. They find that the gain in speed with increase in illumination is greater in general at low levels of illumination, when smaller details are to be discriminated, and

when brightness contrasts are low. Increase in the size of the object to be discriminated seemed to affect speed more than corresponding increase in illumination.

Hecht (58) has reviewed data on the variation of visual acuity with changes in intensity of illumination, and has showed that all are consistent with Koenig's explanation that the sigmoid form of the curve is due to the rods functioning at lower illuminations; the cones, functioning at higher illuminations, give a steeply rising curve which becomes horizontal when they reach their maximal activity. Hecht amends this explanation by postulating a statistical distribution of the functioning organs, taking into account the probable different thresholds of the individual rods and cones. With increase in illumination, it may be assumed that the thresholds of a larger number of sensitive elements are passed, and acuity is correspondingly improved. Hecht also finds in this data confirmation for his theory of a pseudo-reversible bimolecular photochemical process and for his calculations as to the minimal retinal area which must be stimulated if the absolute threshold is to be passed. Lasareff (87) also makes certain assumptions concerning the number of cones which must be stimulated to produce a liminal sensation when he deduces a formula from his quantum-ionic theory to represent the relation between the absolute brightness threshold and acuity as measured by the least distinguishable separation between two points. Experimental data showed agreement with the formula.

Freeman (41, 42) has found that under certain conditions the Aubert-Foerster phenomenon is reversed, and that under other conditions the relations between the sizes of test objects and the distances at which they are just visible are related in accordance with the laws of geometrical optics. Apparently the phenomenon is an artefact of method. When stimuli whose images fell in the periphery were illuminated for only 80 sigma with either white or colored light, objects subtending the same visual angles appeared equally visible. In the case of foveal perception, however, the ratios of threshold visibility were not proportional to actual size ratios when colored stimuli were used, and the ratios differed for different colors. Apropos of this observation, it may be mentioned that Pech (105) has recently reminded us that the properly corrected eye can only be emmetropic for the brightest part of the spectrum, and must necessarily be hyperopic for the longer wave-lengths and myopic for the shorter wave-lengths. Miles (97) describes the brilliant coloration of objects viewed through a prism held flat against the bridge of

the nose, and he observes that details are thus submerged and masses accentuated.

Bordier (7) explains how moire effects arise from the superposition of one mesh of parallel lines upon another. Since the phenomenon depends upon the presence of diffusion circles, it could not be observed by an eye with ideally perfect acuity. This same blurring at acute angles which is associated with slight imperfections of acuity makes the Maltese cross a sensitive test for astigmatism, according to Beach (4).

Discussion of photometric methods is of fundamental importance in relation to research in the field of vision. In a note of a page and a half, Smith (120) has pointed out the fact that although two photometric fields, A and B, may be equated and then, in a series of similar matches, substitution may be made alternately on one side and then the other so that the series is of the order: $A=B$, $B=C$, $C=D$,, it is nonsense to suppose that A will equal the last term of the series in hue and brightness. Campbell (11), in answer, points out that "equality" in measurement is not a transitive relation, and remarks that, although the theoretical difficulty might not be recognized, no one would, in practice, use such a photometric method as the one described. Richardson (113) answered the same note with a reminder that the range within which equality is judged to be such varies with a second stimulus when the first is kept fixed.

Crittenden and Taylor (18, 127) have sponsored an ambitious project in the endeavor to standardize methods of heterochromatic photometry. Six laboratories co-operated by measuring thirteen colored filters whose integral transmissions had been calculated from spectrophotometric measurements and standard visibility data. Although this method yields best agreement, it was hoped that the interlaboratory comparison would justify the general adoption of some more simple and feasible means of measurement. It was found that averages of determinations made by the equality of brightness method approached those found by spectrophotometry but results could not be considered conclusive, especially since observations made with different photometer heads gave varying determinations even for the same group of observers. When the flicker method was used, certain systematic differences appear in the determinations made by different laboratories. These differences showed no correlations with individual differences in color sensitivity as tested by the Ives' solutions. It seems not unreasonable to hope that the sources of these

systematic differences may be discovered. At present, results scarcely justify a preference for either of the substitute methods.

The question of the optimal fields for photometric comparisons seems to have inspired the interest which Heinz and Lippay (61, 62) have shown in the relation between ability to discriminate and magnitude of the retinal area stimulated. They find that discrimination improves with increase in the size of the image, but not regularly. At the macula, there was an increase of more than 100 per cent in discriminability between fields subtending respectively 0.15 and 0.26 mm.; in the periphery of the retina, the same increase in field produced nearly three times that increase. The authors conclude that within limits, the observer's ability to discriminate is a function of the number of sensory elements excited. Martin and Richards (93), working with a black object exposed on a light ground, studied the effect of varying the brightness of the surrounding field in order to determine the optimal size of surrounding field in binoculars. They found that a smaller field was advantageous when the field was brighter than the ground; a larger field when it was darker. Cobb and Moss (13) devised an experiment to determine the effect of a dark surrounding field when the observer was concerned with a lighted area of varied dimensions. Performance was found to be more accurate when the lighted field subtended an angle of 9 degrees or more but there was little further gain when the angle subtended was increased beyond 16 degrees. Löhle (89) has been interested in the relations between magnification, and extent and brightness of field as determinants of the efficiency of telescopes used in making observations at low illuminations. He concludes that the efficiency of telescopes having equal objective openings increases as the square root of the magnification; with equal magnification, efficiency increases as the square root of the area of the entrance pupil. This accords with his observation that twilight acuity depends upon brightness contrasts. However, Kühl (85) finds quite unacceptable Löhle's suggestion that discrimination varies with the square root of the subjective intensity of illumination. He offers a formula in which acuity is shown to be a function of the logarithm of the intensity of illumination. This seems to fit both Löhle's data and his own, and also mediates the transition between Riccò's and Piper's laws. In a more recent article, Löhle (90) reports data showing that when threshold intensity values were determined for two observers, Piper's law was found to hold for angles from two to seven degrees,

while Riccò's law holds for angles up to ten minutes both for foveal and extra-foveal vision.

Differences in the functioning of different parts of the retina have been the subject of a number of studies. Weymouth and his colleagues (135) found a uniform decrease in sensitivity from the fovea centralis to the periphery. No differences were found which could be associated with the anatomical peculiarities of the fovea centralis, rod-free area, macula, or non-vascular area. The decrease in sensitivity is more rapid in the vertical than in the horizontal meridian. Kleitman and Piéron (76), investigating differences in the rate of superficial summation of light impressions by determining the least perceptible brightnesses of four circles subtending angles varying from 6 minutes 10 seconds to 61 minutes 40 seconds, found that the rate of superficial summation increases as these surfaces are farther from the fixation point. At 60 degrees, under conditions of both light and dark adaptation, summation is practically integral; at the fovea, negligible. Particularly in light adaptation, absolute sensibility increases on approach to the para-foveal region. Results were the same for white and for green light. For red light, in light adaptation, summation was practically integral except for the region within 10 degrees of the fovea; in dark adaptation, summation in the para-foveal region was greater than for other lights but less than the summation for red under light adaptation.

Ferree (31) shows how the higher critical frequency in the periphery as compared with the center of the retina explains a phenomenon reported by Gradle (51) and amplified by Gaehr and Packard (44). The former had noted that the rotating propeller of an aeroplane was invisible, or a blur, when viewed directly, but that the rotating sectors are visible with eccentric fixation.

Renquist and Malin (110) rotated screens of complementary colors, red or yellow before one eye and green or blue before the other, and then timed the duration of rivalry with dark and with light adaptation, with central and peripheral observation, and with fields subtending visual angles of 2, 4.5, and 13.5 degrees. Rivalry persisted longer in all cases for light adaptation, and for the center as compared with the periphery. For the 2 degree field it endured longest, but for the 4.5 degree field it ceased more quickly than for the 13.5 degree field. The writers ascribe this fact to the inhibiting effect of the rods upon the functioning cones, which latter they conclude are responsible for the persistence of binocular rivalry. The relative absence of this phenomenon in the periphery is also ac-

counted for in terms of functional advantage: differences in peripheral images are frequent under normal conditions and must be ignored, hence any tendency to rivalry would be inhibited.

Creed and Granit (17) have measured the latency of negative after-images after 15 seconds' fixation. They find a maximal latent time at the fovea, decreasing regularly to about 2 degrees excentric, rising to a new maximum between 2 and 3 degrees, with another steady decrease following. They associate the second maximum with the replacement of the cones by the rods as the dominant functioning organs. They find, also, that the latent time depends on the latent time for that part of the retina where the margin of the disc falls, the rest of the image immediately filling in in a fashion analogous to the supposed filling-in of the blind spot. It may be noted here, however, that Helson (63) has undertaken to upset some of the familiar theories about the blind-spot, and offers experimental evidence for his view that the blind-spot is in some degree sensitive to light stimuli.

Kleitman and Blier (75) have studied color and form discrimination in the periphery. When they investigated the limits at which each color could be recognized with certainty by moving one of six colors in from the periphery until the observer identified it, the blue field appeared to be largest, white and red about equal, yellow, green, and gray smaller and approximately equal. But it was shown that the possibilities of confusion influenced the determinations, for the limits were increased and white became about equal to blue when the last three colors were eliminated from the test. Yellow also showed a larger field when the test was made with only red, yellow, and blue. When the form sense was similarly tested with circles, triangles, squares, and five-pointed stars each of five different sizes, it was found that discriminability varies as the size but shows no significant differences for the different figures. The unequal decrease in sensitivity from the fovea outward in different meridians was noted. Fazakas (29) investigated the chromatic sensitivity of the peripheral retina by determining the locations of the thresholds for colored circles of various dimensions on a black ground. At any given point, sensitivity appears to be greatest for red and yellow, less for green, and least for blue. Isopters are elliptical and parallel, and about equidistant for each one minute increase in the diameter of the test object from 10 degrees to 65 degrees on the nasal side. Fazakas points out some inadequacies of the duplicity theory and suggests that the greater sensitivity of the nasal part of the retina

may be due to its greater functional importance. Ferree and Rand (34) find that color fields mapped with red, blue, and green Heidelberg pigments on neutral gray, are less extensive than the form fields and show a greater range of variation for different individuals. Individual differences in color sensitivity and the greater effect of the judgment factor account for the latter fact. For the different colors, the average meridians were, in descending order, white, blue, red, and green. When the range of individual variations was considered in relation to sex, age, and refraction (35), it appeared that sex differences are not significant, and that age does not appear to be a factor below forty years. Fields of myopes and presbyopes ranged smaller than those of emmetropes and hyperopes. Gaudissart (46), following the suggestion that colored stimuli give smaller fields than white of the same size, presents data showing how this reduction in fields is effected either by reducing the size of the white or by substituting colored stimuli.

Adams'(1) comprehensive review of the literature on dark adaptation considered in all of its phases has been announced in several reviews as indispensable to anyone interested in related problems. The material is organized so as to furnish a ready guide to particular aspects of phenomena of dark adaptation. Some seven hundred titles are included.

Møller (98) has used Tscherning's photometric glasses in investigating phenomena of dark adaptation. He determined the apparent brightness of an object by comparing it with a surface of constant size and illumination which could just be seen through the densest glass after maximal adaptation, using as a measure the glass necessary to effect apparent equality. All of the adaptation curves which he plotted from his data show a regular rise, without any transitions such as the author claims would be expected if two different factors, such as different rod and cone functions, were involved. He finds that the time required for dark adaptation after exposure to a given light is the same as that required for "disadaptation," *i.e.*, for reaching the original state of light adaptation. He finds no binocular summation of stimuli, and claims that the two eyes adapt independently. For maximal adaptation, the visibility of objects of moderate size is found to be a function of brightness and area,—the area of the retinal image and of the threshold brightness yielding a constant product. Red adaptation is difficult to demonstrate except under optimal conditions. Møller finds that when red is no longer visible, a central scotoma is also present. He associates

this relationship with absorption of the short wave-lengths by the macular pigment. Since the red rays are not visible, and the short wave lengths are absorbed, the sensitive elements of the macula are not stimulated. He also explains the reversal of the Purkinje phenomenon in the fovea at low illumination as due to the macular pigmentation, not to the absence of rods. He demonstrated peripheral adaptation by means of an acuity test and also showed the decrease in acuity with decreasing illumination. All measurements are expressed in units representing the Tscherning glasses used in making the determinations. Edmund (26) used the Tscherning glasses in investigating the duration of luminous impressions under different conditions of adaptation. An ingenious method of measurement showed that the impression lasted 0.03 second longer for each reduction of brightness by one photoptry, *i.e.*, with use of the next denser glass.

In their study of the effect of dark adaptation on the critical frequency of flicker for different illuminations, Lythgoe and Tansley (92)² found a fall in the critical frequency at the fovea during the first ten minutes or so of adaptation, while in the periphery there was a fall with high illumination of the test patch and a rise for low illuminations. The fall is attributed to the dark adaptation of the cones; the rise, to the rods. When the brightness of the surrounding field was progressively decreased, the results showed a similarity to those obtained during prolonged dark adaptation. The authors conclude that a mild dark adaptation is induced by a decrease in the illumination of the surrounding field, while an increase causes a corresponding light adaptation.

Heinemann (60) reports a study of the differences between momentary adaptation and adaptation after prolonged exposure. Subjects were adapted to a given intensity and then the experimenter recorded the time which they required for recognition of a stimulus of 1/300, 1/600, or 1/1200 the brightness of the light to which they were adapted. He found that this period increased with longer adaptation to the first light, and with decrease in the relative brightness of the test light. It was shorter for the moving eye than for the fixating eye, and shorter with binocular than with monocular vision. Children were slower than adults to adapt to high illuminations, but adapted to low illuminations more quickly. Children adapted more quickly than adults to green and blue; more slowly to

² Adams' (1) review of this article is the source of the comments upon it made here.

red. Apparently the Purkinje phenomenon is more marked in youth. Heinemann suggests taking as a quantitative measure of the Purkinje phenomenon the ratio of the brightness of the blue to the brightness of the red which requires the same time for adaptation. He explains his observations in terms of Müller's theory of the inhibiting effect of the functioning cones upon the rods.

Lasareff (88) presents data showing that the sensitivity of the dark adapted eye is low in childhood, increasing to a maximum at about eighteen years of age, and then gradually decreasing until at the age of eighty, only about 25 per cent of the maximal sensitivity is retained. He shows that this empirical data is in satisfactory agreement with the sensitivities calculated from a formula derived from his theory that there must be a constant concentration of a hypothetical sensitizing substance to maintain function in the nerve cell and that this substance is present in different amounts at different ages, so that only a portion of the nerve cells present are capable of functioning in very early and very late life.

Siegfried (117) claims to have disproved the alleged harmful effect of ultra-violet rays on adaptation by showing that with equal periods of exposure, a 50 candle-power light with the ultra-violet rays cut out impairs adaptation more than the concentrated ultra-violet rays of a Bach lamp.

Parsons' (104) casual remark that the eye is adapted to utilize the rays in the region of the maximum of the sun's emission has been criticized by Smith (121) who points out that when the curve of emission is plotted in terms of frequency rather than of wave-length this fact does not appear to be true. Hartridge (54), in opposition to Smith, claims that there are no adequate grounds for regarding frequency as more significant than wave-length. Pokrowski (109) offers another explanation for the lack of coincidence between the sensitivity curve of the eye and the curve of energy emission of the sun in the region of the visible spectrum. He suggests that the eye developed under conditions where not direct sunlight, but light reflected from the green grass and leaves determined the course of development. The sensitivity curve fits in very well with this hypothesis. Hauser (56) has claimed priority for this suggestion, since he had suggested earlier that the differences between peripheral and foveal visibility curves are due to the influence of vegetation in affecting the intensity distribution of the effective light of the sun.

Parsons' (104) reference to rods as the organs of twilight vision

and to cones as functioning in photopic vision has been criticized by Forbes (37), who maintains that this is no longer obvious. The presence of color sensitivity in the middle range of adaptation where the rod function must be assumed to be dominant, seems to indicate that there is some photochemical substance involved whose distribution is not necessarily coincident with that of rods or cones. Edridge-Green (27) also took the occasion of Parsons' brief article to criticize the current version of the duplicity theory, offering his own explanation that the rods are not percipient elements but control the visual purple which in turn sensitizes a photo-chemical substance surrounding the cones, and stimulating them as it decomposes.

In discussing the Purkinje phenomenon, Jaensch and Stallman (67) present a different type of explanation. They undertook to determine whether the presence of different levels of experience can be demonstrated in the sensory realm. They concluded that a disintegration of the more developed functions, and a reversion to a lower level of experience is apparent in vision when observers are fatigued. The change in brightness values represented in the Purkinje phenomenon are considered to occur at this lower level of functioning, for the Purkinje phenomenon is absent when accurate discrimination, involving the later developed functions, is required. The Purkinje phenomenon, then, is considered by these authors to represent the dominating activity of the primordial functions and the relative inactivity of the later developed functions. Jaensch (66) also suggests that the Purkinje phenomenon may have a teleological explanation, since Rayleigh has shown that night light contains a larger proportion of short wave-lengths than daylight, and consequently the dark adapted eye is better prepared to make the best use of this changed distribution of the components of natural light. He finds this functional explanation quite consistent with the duplicity theory, although he is inclined to believe that twilight vision represents a more extensive system of functions than mere rod vision.

Gross (52) employed various methods in establishing his conclusion that the Purkinje phenomenon is absent at the fovea. He found that when he used Vogelsang's method of *Momentadaptation*, it was very difficult to maintain fixation, and he attributes Vogelsang's contrary results to failure in this respect. Gross found that the area in which the Purkinje phenomenon appeared to be absent corresponds very well with the rod-free area. He suggests that several two-factor theories may account for his conclusions: von Kries' duplicity theory; Tschermak's modification of the Hering theory; absence of

visual purple in the rod-free area; different concentrations under different conditions of adaptation of two photochemical substances of different absorption curves.

A posthumous report of an experiment by Broer (8) indicates that the Purkinje phenomenon is independent of dark adaptation. Using stimuli which, exposed together under conditions of light adaptation, gave after-images of red and blue of equal brightness, Broer found that a Purkinje phenomenon appeared when these were observed projected on a light screen in a dark room, although the observer was not dark adapted. Rosenberg (115) has investigated the range of illumination at which the Purkinje phenomenon is apparent in different regions of the retina. He concludes that within these limits the rods and cones function together, while below the lower limits, about 1/80 lux, the rods apparently function alone; the cones functioning alone above the upper limits of the Purkinje phenomenon. He found no evidence of this phenomenon at the fovea, but it was found with the stimulus three degrees excentric, and continued at higher illuminations as the stimulus was moved further into the peripheral field.

Sloan (118) has investigated the Purkinje phenomenon by studying the changes of the visibility curve with systematic variations in intensity of light, size of photometric field, and state of adaptation of the eye, making determinations for twelve spectral wave-lengths. She used a method of heterochromatic photometry, exposing the test field for two seconds. The point of maximum visibility was found to shift only below an intensity of 0.2 meter-candle. The Purkinje effect was always apparent on a change from light to dark adaptation when other variables were kept constant. Decrease in size of field increased relative sensitivity to short wave-lengths except under conditions of dark adaptation at low intensities, when the effect was reversed.

Several experiments are reported which have to do with phenomena of chromatic adaptation. Almack (2) has measured the loss of sensitivity to spectral red, yellow, green, and blue lights, equated in brightness, in saturation, and in energy, after stimulation for periods between 2 and 300 seconds, under conditions of light and of dark adaptation. By systematically varying the conditions, she has shown the effects of the four variables involved: wave-length, intensity, and duration of stimuli, and state of adaptation of the eye. Almack finds that the loss in sensitivity is greater as the original sensitivity of the unfatigued eye is higher. She regards saturation

and brightness as effective factors only as they are functions of intensity. She suggests that the curve for chromatic adaptation represents the arousal of some complementary process, since subliminal stimuli are sensed first as of the complementary color, at higher intensities as colorless, and finally as of their actual hue.

Gasteiger (45) plotted dark adaptation curves after twenty minutes pre-exposure to spectral red, orange, green, and blue of equal physical intensities. He found that initial sensitivity was highest after adaptation to red, lowest after adaptation to blue, with orange and green intermediate. Since the subjective brightness of red was appreciably greater than that of blue, subjective brightness apparently was not a factor. Gasteiger concludes that the cones must play a part in dark adaptation, since only they show differential response to different wave-lengths.

Comparing his data on the course of brightness decrease in chromatic adaptation to spectral lights with the implications of Lasareff's theory, Kravkov (81) finds that these data support the theory, yielding exponential curves with the exponents showing a linear increase with increase in the intensity of the stimulus. The curves differ for the three different wave-lengths used; violet was more fatiguing than red, red more fatiguing than green.

A different aspect of brightness relationships has interested Hiecke (64). He offers a formula for calculating the brightness of a mixture of monochromatic lights. The chemical law of mass effect affords him an explanation, by analogy, of the fact that a mixture of red and green lights never has a brightness value equal to the sum of the separate brightnesses of the component lights.

Stainton (125) has investigated the phenomenon of Broca and Sulzer. He had observers adjust the brightness of a standard field until a photometric match was obtained with a flash enduring through varying fractions of a second. The subjective brightness of the accurately timed test stimulus was measured in terms of the objective standard seen as equal. Stainton concludes that the rise of sensation is primarily a function of intensity, and shows little variation when different spectral lights and white are used as stimuli.

Lowry (91) has investigated the differences in brightness sensitivity with binocular and monocular vision. He found that contrast sensibility is much greater with binocular vision when the observation is made at a brightness level of below 50 milli-lamberts, but that binocular and monocular vision give more nearly equal results above this level. By using an inverse method of threshold determination,

Lowry again demonstrated the greater sensitivity with binocular vision, especially with low illuminations of the surrounding field. Zigler and Ward (138) found phenomenal differences between uniocular and binocular vision similar to those reported earlier by Katz: uniocular vision gives rise to an impression of lesser brightness, indefinite localization, increased distance, reduction of hard surface characteristics, and it reduces the tendency toward objective reference.

Fedorow and Fedorowa (30) find that brightness curves plotted from measurements made after stimulation by intense red or green spectral light resemble those of protanopes and deuteranopes. They suggest that their method might be applied to the investigation of the normal sensation curves for red, blue, and green, since it seems that a temporary color-blindness can be induced in a limited retinal area. They plotted their curves by comparing the brightnesses of neighboring points at equal intervals throughout the spectrum. They explain a quirk in the curve in the blue as due to macular pigmentation and fluorescence of the ocular media in the blue and violet. A redetermination of the tri-chromatic coefficients of the spectral colors has been undertaken by Wright (137). Ten observers took part in the experiment, each making about thirty matches throughout the spectrum after five to ten minutes dark adaptation. Wright attempted to eliminate individual differences by having observers match the three selected colors to monochromatic lights. In this way he determined factors for correcting for individual differences.

Fatigue induced by stimulation by white light of three widely different intensities during periods varying from five seconds to two minutes, has been measured by Geldard (47). Fatigue effect is plotted in terms of the percentage of illumination of comparison light required for a match as compared with that required for a match before fatiguing. With more intense stimulation, curves fell more steeply in the first twenty seconds and reached a lower lever,—*i.e.*, less light was required from the comparison light to effect a match. A further study (48) showed that the fatigue of a limited area of the retina effects an increase in sensitivity in adjoining areas, since more light was required for a match when the fatiguing light was observed by one eye, the test light by the other. Geldard's apparatus was used by Johannsen (68) in a study of recovery from visual fatigue. She found that recovery is a function of the duration and intensity of the original stimulation, and is much quicker for some observers than for others. Wirth (136) suggests that certain differences in sensitivity may be measured by investigating

the relative effects of after-images immediately following brief fixation. He determined the objective brightness difference necessary to maintain subjective equality between the red and green halves of a circle after five seconds' fixation, and also determined what effect was produced in the apparent saturation of one color after five seconds' fatigue to its complementary. Measurements are given in terms of the percentage of contrast color which must be added to the original color when it is observed after five seconds' observation of its complementary, if it is not to appear more saturated than the same color observed without such previous stimulation.

A number of other investigations have also been reported concerning the after-effects of stimulation as represented in after-images. Warren (134) recounts an experience in which a vivid after-image appeared after an intervening sleep of ten to fifteen minutes. The image underwent changes which duplicated the course of the original experience. BurrIDGE (10) extends to after-images his explanation that functional changes persisting after removal of the excitants may be due to mediation of the excitability by processes of colloidal aggregation which develop at variable speed and subside in their own time. Frehafer (43) has acted upon Forbes' (39) suggestion that the merits of proposed standard whites might be investigated by observation of the first after-image, in which there appears, under favorable conditions, a flash of color of higher saturation than that of the original stimulus. She varied the color temperature of a standard light source by means of a rheostat, but found no point at which the after-image consistently appeared colorless. She found that this after-image failed to appear at the fovea centralis.

Judd (69) has suggested that the Purkinje phase of after-images, as well as the blue arcs of the retina, may be due to visible radiations from active nerve fibers. Using spectral lights of different wavelengths as stimuli, Judd found that there is a definite rise in intensity of stimulus necessary to evoke the blue arcs with increase in wavelength of the stimuli. He is inclined to believe that at low illuminations, the blue arcs are consequent upon activity initiated by the rods. In the main, of course, Judd is following Ladd-Franklin's suggestion (86) that the blue arcs are due to luminosity of the excited nerve fibers. Ellis (28) found no evidence for this view when he stimulated frogs' retinæ electrically. He concluded that the objections presented to the alternative electric theory of causation are not cogent, and he shows how this theory is competent to explain the observed after-images of the blue arcs.

In a series of three papers, Ebbecke (23, 24, 25) presents conclusions based on his repetition and extension of experiments with after-images. He suggests that the brightness of background against which an after-image just disappears may be taken as a measure of the strength of the image. With suitable projection fields he has succeeded in showing that positive and negative after-images endure equally long. Emphasizing the necessity of distinguishing between duration of visible images and duration of physiological after-effects of stimulation, Ebbecke cites evidence indicating that the latter endure so long that permanent scotomata induced by exposure to direct sunlight may be regarded as limiting cases of this phenomenon. The disappearance of the observable image is explained as due to adaptation. The second paper describes the *positives Hellbild* which Ebbecke observed when the background was brightened suddenly. He explains this phenomenon of a positive after-image appearing on a bright projection field in terms of the supposed enduring effect of stimulation and the antagonistic adaptation process which he assumes must be occurring somewhere in the nerve pathway. This same hypothetical antagonism is offered as explanation of the spontaneous fluctuations in the after-image: a positive after-image would be present when the retinal after-effects of stimulation are the dominating factors in the experience; a negative image, when the adaptation processes occurring at a higher level are in the ascendancy.

Phenomena observed in studying rivalry in after-images has demonstrated, according to Gellhorn (49) that concurrent or previous stimulation of one eye affects the other through some intracortical process. He found rivalry more pronounced when the stimuli for the two eyes showed marked differences in intensity or hue. That color dominates which contrasts most with the brightness of the background. Twenty seconds' fixation appeared most favorable for the appearance of rivalry. The gestalt factor failed to influence rivalry in the after-images. When the projection screen was observed monocularly after fixation under conditions favoring the appearance of binocular rivalry, the phenomenon appeared. The image of the observing eye tended to dominate except when the factor of brightness contrast was employed to favor the dominance of the image of the non-observing eye; by this means the latter image could be made to dominate.

Karwoski (70) has found that after-images following stimulation by spectral lights of various selected wave-lengths show characteristic variations in hue with changes in the intensity of the stimuli.

Only at moderate intensities was the hue of the complementary favored; at higher intensities, purplish hues appeared. Karwoski offers the explanation that above moderate intensities, subsidiary processes are aroused in relative disproportion, the dominant process having already reached its maximum. Consequently, after-images from stimuli of high intensities show hues complementary to those associated with these subsidiary processes.

Problems associated specifically with color vision are numerous enough, and sufficiently productive of controversial discussion, to claim a literature of their own. Omitting studies of anomalous color vision and color vision in animals, there remains a body of literature almost equalling that which has been reviewed above as referring to all of the other aspects of the specifically sensational aspects of vision. Andrade (3) is the author of a simple exposition of the fundamental facts of color vision and color measurement. A book by Hillebrand (65), published since his death with emendations by his wife and former student, presents a fuller exposition under the headings: I, the dependence of color sensation on stimulus; II, the dependence of color sensation on sensitivity; III, color theories. In the last section, Hillebrand has pointed out the advantages and defects of both the Young-Helmholtz and the Hering theories. Tschermak (132), on the other hand, shows a decided preference for the four-color theory, emphasizing its more adequate explanation of certain facts where the tri-color theory is generally admitted to fail. He admits the necessity of extending and modifying the present four-color theory, however, suggesting that a distinction between receptor organs and conducting apparatus with different influences attributed to each, would help to explain variations under different conditions of adaptation, the independent variations of brightness and color changes in after-images, and other phenomena which now present difficulties for this favored theory. Ladd-Franklin's (86) published work on color theory has been republished in book form with a few emendations and together with some discussions by other writers. A criticism of the Ladd-Franklin theory is offered by Müller (99), who remarks that Mrs. Franklin's request for his opinion has finally persuaded him to publish his objections. These are presented under six headings, and as briefly as is consistent with clearness. Birren (6) and de Gourmont (20) are authors of recently published books having to do with color phenomena, but I have found neither the original articles nor reviews available.

The position of gray in the color pyramid has been discussed by

Dimmick (21) and by Dimmick and Holt (22). Dimmick warns against allowing physical and physiological consideration too much influence in determining the psychological conception of the color pyramid. He calls for due consideration of the psychological uniqueness of red, green, blue, yellow, white, black, and gray; and of the fact that in experience hue, brightness, and saturation do not vary independently. He criticizes two recent theories proposed by Rich and Forbes. In answer to a criticism of their earlier report on the relation of gray to black and white, Dimmick and Holt carried through an experiment designed to force uninitiated observers to develop color categories of their own by requiring them to recognize color changes due to varying proportions in mixtures. Observers tended to drop out orange and purple and to include gray as they became more proficient, and all found it necessary and sufficient to include as unique categories red, yellow, green, blue, black, white, and gray. Gertz (50), extending the mechanical analogy of the color triangle, suggests that the arousal of a white sensation by the compounding of conditions for the arousal of various color sensations may be compared to the transformation of different forms of energy into heat. He would regard white as being the lowest form of vision and so would explain the frequent retention of achromatic vision when chromatic vision is pathologically destroyed. Österreich (101) describes a number of demonstrations of the fact that mixing yellow and black gives rise to a sensation of green. He regards it as an evidence of the inadequacy of color triangles and other schemes representing color relationships that they fail to indicate this phenomenon. Kiesow (74) has answered this contention by maintaining that this green is a product of psychic synthesis. Apparently he claims that a mixture of yellow with black or gray gives brown.

In a communication to the Academy of Sciences, Ostwald (102) has expressed his views on the fundamentals of chromatic measurement. Matthaëi (94, 95) has used the colors in the Ostwald color circles in his experimental attempts to study hue differences in isolation from subsidiary attributes. In the first part of his paper, Matthaëi explains how his measurements of the brightnesses of the different colors in the Ostwald circle indicates that the circle includes three groups of colors of different brightnesses. He concludes that the apparent brightness values of colors under dark adaptation is a function of cortical processes. In constructing a color body from the color circle, Matthaëi makes the brightness attribute the ordering principle. Nyberg's (100) scheme for construction of a pigment

color solid is made without reference to any physiological hypothesis. A critical review of Ostwald's color theory, based upon mathematical grounds, opens the way for the new theory which Nyberg presents.

Schubert (116) has demonstrated the weakness of one major point in the Ladd-Franklin theory, by showing that under conditions of neutral adaptation, suitably chosen spectral red and green mix to give a colorless light. The validity of the judgment was established by Herings' method of successive contrast, and by equation with another binary mixture itself verified by the Hering method. Schubert found characteristic differences in color sensitivity among different observers, and different color tones appeared in duplicated mixtures under slightly different conditions of light adaptation. Light adaptation at different times of day, and under different circumstances of brightness or cloudiness, gave rise to different judgments.

Hecht (57) describes an easy method of producing binocular fusions of colors. He concludes that the uniformity with which his observers reported the appearance of yellow when they had a red filter before one eye and a green filter before the other, shows that the fusion of these colors to produce yellow must occur at some higher level than in the retina. He regards this demonstration as a further vindication of the tri-color theory. Rochat (114) claims to have demonstrated the subjectivity of contrast colors by showing that appropriate selection of the surrounding field may give rise to the appearance of a contrast color in a gray field illuminated by a mixture of monochromatic complementaries when the induced color does not enter into the composition of the gray field. The same contrast color appears when the neutral field is illuminated by a mixture of monochromatic lights and when it is illuminated by natural white light. Focht (36) explains her contrary observation that color contrast is absent except when white light is present, as evidence for the view that the contrast effect is due to the failure of a lesser stimulus to evoke response when a stronger stimulus is present. For instance, when white is presented in a green field, the green component in the white fails to arouse response and consequently the white takes on a reddish tinge, according to her explanation.

Van Heuven (133) has investigated the effects of simultaneous contrast for different colors under different conditions of illumination and for different degrees of saturation, by measuring the inhibition of objectively measured irradiation effects. He found that irradiation phenomena stand out best in white light, less in blue and green. Although these values do not hold for subjective judgments, here

again red values are least. Simultaneous contrast values are found to be less for colored than for white light, also differing less for different intensities. Following up his own demonstration that chromatic adaptation proceeds at different rates for the three basic colors of the Young-Helmholtz theory, Kravkov (83) explains his observation that there seems to be a disproportionate impression of red present in cases of successive contrast as due to the unequal rates of fatigue to the different components in the original experience. The successive contrast colors he would regard as complementary, not to the original stimulus, but to the color-tone which this assumes after chromatic adaptation. Tennant (128) warns against the disregard of the higher cerebral processes in explaining facts of color contrast. He points out the part played by fatigue due to long fixation, and the influence of perception of the total situation, the differences between absolute and comparative judgments, and the possible effect of suggestion. He indicates the inadequacies of both Helmholtz' and Hering's views, and emphasizes the necessity of taking account both of physiological retinal processes and of higher cerebral processes.

Collins (16) finds that the Rayleigh color equation, derived by mixing spectral lights, is also valid for mixtures produced by rotating pigment discs, and equated with yellow, blue, and white. The results of two hundred observers were used to establish her conclusions. The difficulties of judging the proportions of the different components in a mixed color are indicated in the work of Richardson (112) and Maxwell (96), who asked observers to indicate their estimates of the redness of different pinks by giving them places on a scale in which 0 represented white and 100 represented scarlet. Experience and exposure sequence seemed to influence judgments. Maxwell claims that he found some correlation between estimated value and per cent angle of red in the rotating discs.

Several German investigators are concerned with the problems of *Farbentransformation*, *Kontrast*, and *Helligkeitskonstanz*. Kravkov (82) claims that the action of a central process, effecting a compensatory response, must be invoked to explain the observation that a white surface brightly illuminated by a colored light appears less colored than the physical conditions would lead one to expect, while objects similarly illuminated undergo color changes such as would result from an admixture of the color complementary to that of the illuminating light. Haack (53) discusses the question of the identity of color transformation and contrast effects. She submits

the argument that phenomena of contrast are almost completely independent of intensity of illumination, while phenomena of transformation show a regular variation with changes in intensity of illumination, as evidence of the independence of the two phenomena. She seems inclined to adopt Katz' view that simultaneous contrast is peripherally conditioned; transformation being due to central factors. Kravkov and Paulsen-Bachmakowa (84) find that color changes due to illumination by colored light do not induce a contrast effect. This again indicates that there is an essential difference between contrast and color transformation. Katona (71), investigating the relation between experience and the apparent brightness of objects under different illuminations, finds that familiarity with objects favors *Helligkeitskonstanz* but is not a *sine qua non* of that phenomenon. The phenomenon is only present, however, when different illuminations or different objects under the same illumination make comparisons possible.

An article by Krauss (79) is the one first selected for criticism by Katz in his *Sammelreferat* (72, 73) on reports of research concerned with perception of color. This review represents really a series of searching criticisms of work reported within the past three years by Krauss, Bocksch, Ehler, Friedler, Granit, Kardos, and Brunswick. Only the work of Ehler and of Brunswick win even qualified approval from Katz. Krauss (80) has replied to Katz' criticism with the reminder that Katz neglected to consider several points which Krauss had regarded as of especial importance in the original work, but Katz denied, in turn, that Krauss' answer in any way detracted from the significance of his criticism.

A phenomenal study of relations of transparency, form and color has been made by Tudor-Hart under the direction of Koffka (77). She measured the effect upon the appearance of yellow circles viewed through a blue episcotister, of exposing a gradually increased number of similar circles outside the field of the episcotister, by measuring the decrease in the opening of the episcotister which was necessary to make the circles appear a neutral gray, as they did when no yellow circles were freely exposed. She also investigated the apparent transparency of the episcotister with relation to its brightness differences from a varied background. Mintz (78) has also contributed a study on brightness effects undertaken under Koffka's direction. He concludes that Weber's law may hold under very simple conditions, but that many other factors than intensity of stimulus contribute to recognition of differences in brightness. Using the same brightness

in different constellations, he finds support for the contention that effects must be explained in terms of the total stimulus situation. Brown and Hoisington (9) report a study in which observers reported the apparent presence of color in a surface exposed through an opening in a uniformly illuminated screen. The exposed color was varied through five brightness steps. Two observers showed a tendency to report color whenever there was a brightness difference present. Light colors were reported more frequently when the test spot was brighter than the screen; red, green or blue were reported when the spot was darker.

Several writers are endeavoring to supply the recognized deficiencies of the classical theories of color vision by offering new hypotheses. Rich (111) offers an eclectic theory, modeled after the Ladd-Franklin theory, but making gray the center of the visual system and black and white a disappearing pair. He postulates a primitive color molecule which mediates gray, and which differentiates with respect to intensity into a portion which responds to all wave-lengths and mediates white, and another portion which, upon spontaneous decomposition, mediates black. The former differentiates into processes mediating respectively blue and yellow, and the latter of these differentiates into processes mediating red and green. Phenomena of contrast and after-images are explained as due to spontaneous decomposition of the split products.

Hecht (59) has undertaken to express certain fundamental facts of vision in formulae. He has shown how they can be explained on the assumption that there is a photosensitive substance which decomposes in the light to form two products, which recombine, under dark adaptation, to form the original substance. He recognizes certain over-simplifications involved in this analysis, and suggests some necessary amplifications of the theory as thus outlined. In discussing color vision, Hecht emphasizes the advantages of the tri-color theory and suggests that three types of cones be postulated to account for the differential responses to the three fundamental colors.

Forbes (40) has analyzed the data for the visibility curve as determined under different conditions by Koenig, Ives, Sloan et al., in an effort to discover what factors must be assumed to play a part in the appearance of the Purkinje phenomenon. He finds evidence for the existence of three or four component processes: two or three photopic; the last, scotopic. Although the data are not conclusive, results arrived at by the different observers all show fair agreement with Forbes' tentative explanation. A more elaborate theory which

Forbes (38) suggested earlier, represents an attempt to meet the objections raised to previous theories and to take account of all familiar facts of color vision, adaptation, after-images, etc., without postulating processes or structures which are not at least analogous to those already known to exist in the organism. Forbes postulated a "visual violet," analogous to the visual purple, but with a different absorption curve and probably present in relatively minute amount. He postulated three different types of cones: a white, a red, and a yellow. Black, green, and blue stimuli he considered inhibitory in their action. The nature of the response is explained as a function of the interference of incident and reflected rays in one of the two critically different parts of the cones, found on either side of a dividing membrane, according to the hypothesis. The acid reaction associated with the bleaching of the visual substance on that side of the membrane where maximal interference occurs, determines whether the region nearer the nerve fiber shall be relatively acid or basic, and, consequently, whether an effect of stimulation or of inhibition shall be apparent. Forbes develops some implications of this theory.

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RECENT RESEARCH IN THE FIELD OF AUDITION

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A considerable amount of interest still appears to be shown in the experimental work carried on among psychologists and their kinsmen in the neighboring fields of physiology, pathology, physics and electrical engineering. So strong is this interest and so productive are the experimental researches that the reviewer is sure of the increased returns which would accrue if some central organization for the exchange of facts and theories were to be set up. This has been accomplished in some measure by the organization of the Acoustical Society of America, but so far its membership is not yet representative of the widest interests in this field. The first issue of its quarterly Journal has made a very real contribution, however, and much good will follow. Within the space limits necessarily assigned to this article it is impossible to do anything more than to sketch in the picture of productivity which is going on in this country and abroad. Many of the papers are presented in such generally inaccessible journals that they were unknown even to the reviewer until he set about the task of summarizing the important acoustic events of the last two years since the preceding summary was published (60).

Theory of Audition. The tendency now seems to swing somewhat away from a modified Young-Helmholtzian theory of analysis by sympathetic resonance and to emphasize for the moment some form of central function, not that practically all of the facts of analysis are relegated to the cerebrum, but that some form of synthesis can only be ascribed to those centers where such organization is neurologically possible. The increasing significance of *Gestalt* in psychology and the neurological correlate of the engram or equivalent form of configuration seems to be a more suitable explanation of our outstanding ability to detect simple tones in a very complex clang. This form of explanation comes from several different directions. We have, for example, the outstanding work of Trimble (73) in the field of sound localization together with his contributions to auditory theory (72). He has raised the question

as to whether the binaural differences, such as the temporal phase and intensive factors, are separately operative. His researches indicate that any one of these physical factors may furnish a binaural difference-pattern that results in localization. Localization would then depend upon the nature of the cortical effects which correspond to these physical differences at the ears.

Another suggestion comes from some of the work done by Bouman and Kucharski (6) on the threshold of masking. They find that when the masking tone is lower than the tone which is masked the intensity of the masking tone can be diminished in direct proportion to the increase in its duration. If the masking tone is higher than the tone that is masked the intensity of the masking tone may be increased in direct proportion to the decrease in its duration. In other words, the masking effect of pitch as regards duration is opposite at the two ends of the tonal range. The results seem to point, moreover, to the importance of the central processes involved in this connection. On account of the large magnitude of the durations employed it is hard to suppose that the effect of masking is entirely peripheral.

In a previous study (5) they had already investigated the effect of giving tones of very brief duration and covering a wide range of pitch from 150~ to 1200~. When a duration was used that gave a nearly constant number of periods, 7.5~ to 8~, a new property of sound began to appear at a certain duration that was somewhat variable with the intensity of sound but was relatively independent of pitch. The general effect was that the old clang disappeared at such short durations and a new clang quality was introduced. The implication again seems to be that the hearing of a clang is not exclusively or even principally explained in terms of the auditory mechanisms in the cochlea but must be referred to the more complex processes of perception relegated to the higher centers. The reviewer is, however, not at all satisfied in the basic logic of such inferences until more evidence is brought forth from the physical point of view as to exactly what happens when the duration of sounds is abnormally curtailed. The effect of very short wave-lengths on inherent resonating capacities of the outer and middle ear should also be scrupulously taken into account.

Through the work of Crowe (15) with cases in which anatomical changes had occurred in the labyrinth secondary to cerebellopontile and brain-stem tumors we find further substantiation of the Young-Helmholtzian theory. Carcinoma in the internal auditory meatus had

destroyed all the cochlea fibers except at the very apex with the result that only tones of $64 \sim$ and $128 \sim$ were heard. The left ear was also similarly impaired for high tones but remained approximately normal within the speech range. This would indicate some form of theory which allocated differences of frequency to separate portions of the basilar membrane.

Tullio (77) has written extensively concerning recent auditory theories, especially as regards the functional relationship of the labyrinth. He agrees with the general trend that assigns tones to the cochlea and noises to the utricular structures. Injury to the semicircular canals causes a lowering of the threshold to auditory stimuli in animals. Sound localization, he believes, is referable at least in part to the semicircular canals. Bonain (4) distinguishes auditory orientation from auditory accommodation and proposes a new theory of audition in terms of the dissociation and diffraction of the auditory waves as they cross the lateral network of the canal of Corti together with a further selectivity of wave-lengths in the tympanic canal. Ultimately the tectorial membrane must produce secondary vibrations that reproduce the primary vibrations of the sound waves. This interference of the reflected wave with the direct wave produces the stationary wave needed to explain sympathetic resonance. The theory is novel and ingenious, but must wait on demonstration and proof.

Pattie (53) has done some further work on auditory fatigue. The observer was first stimulated binaurally with two tones which were sufficiently out of phase to make localization appear wholly on the side with the leading phase. The observer thought he was being stimulated uniaurally. The subject was then stimulated uniaurally on the same side in which phase had led and the degree of intensity in fatigue was measured. These two types of stimulation each lasting for one minute were alternated ten times in one sitting. The results showed that no differential fatigue effect was produced, therefore, the fatigue or adaptation was peripheral. As indicated, the experiment ruled out all subjective factors but the author is aware of a possible loop-hole in connection with fatigue since no third un-fatigued ear was available for comparison. Kuroda (34) reports from Korea concerning a subjective tonal experience without an external stimulus. Five observers reported hearing a definite tone although there were some individual differences as regards pitch and intensity. The question arises: Is this tone peripherally produced or central in origin and what relation has it to tinnitus? If it

is central in origin we may here have an analogy to cortical gray just as the experience of black runs somewhat parallel to the experience of silence.

Banister has given us a competent summary of the outstanding facts and theories in audition in the two chapters contributed to the *Foundations of Experimental Psychology*. In the second chapter he had the able assistance of Hartridge (1). Particular emphasis is placed on the theories of hearing and replies to criticism. All of us are particularly grateful for the clear presentation of Watt's work on volume.

West and Barlow (81) propose a theory of audition that rests almost entirely on the varying kinaesthetic effects of muscles attached to the bones of the middle ear particularly of the stapes which is accountable for sensations of intensity and pitch. The region of the cochlea stimulated corresponds to pitch and is entirely dependent upon the speed of thrust of the stapes. Their theory does not adequately describe either the great number of auditory phenomena such as clang analysis, beats, and combination-tones nor does it conform to the requirements of the all-or-none theory and the facts of refractory phase. But it is certainly original in the sense that the chief emphasis is on the motor aspect of the mechanism in the middle ear.

The Young-Helmholtzian theory receives renewed support from Troland (76) who attempts to square it with the principles of modern neurology. The fundamental factor of selective or sympathetic resonance is now assigned to separate nerve-fiber groups. The pitch of a tone is correlated with the frequency of the neural impulse and the difficulty presented by the principle of refractory phase is met by the cooperation of several fibers when the frequency is higher than the time of recovery. Loudness of tone is correlated with the total number of neural impulses passing through a given cross-section in a given group of fibers. Localization is accounted for by the discrepancy in the time of arrival of the two sounds from the two ears at the cortical level. Brightness comes in as the sharpness of the wave-front in a group of fibers and volume as the number of fibers stimulated. While there are still difficulties in the way, the reviewer believes that Troland has gone farther to explain a number of phenomena in terms of a single theory than has any other recent theorist. Enough validity also exists in his theory to give the complicated mechanism in the ear some legitimate work to do, and finally the modern drift in the direction of *Gestalt* is also well rep-

resented as a cortical affair in terms of complex patterns which account for an organization of unconscious factors.

Meyer (48) has made further contributions toward the function of the cochlea in a paper which frankly rejects what he calls "fantastic 'anatomical' data" by taking into account all that the microscope reveals. His fundamental equation depends upon two essential items: (1) the length or lengths of the phragma involved which may then be translated into the number of sensitive cells stimulated; and (2) the periodicity of their movement. These factors are then dealt with in terms of a hydraulic principle which requires that the basilar fibers be not stretched but merely anchored at their ends to the sides of the cochlea. The theory is carried through an elaborate mathematical exegesis. He also deals critically with experiments that have offered explanations differing from his own and gives a description and illustration of a model which demonstrates his theory.

Sound Localization. The work of Trimble together with his supporting theoretical papers occupies an exceptional position of importance in this field for the biennium which forms the period for this review. Beginning with a paper (73) which summarizes his doctoral dissertation he laid down the criteria for the lateral localization of sounds which were produced by discrete impulses of a very short and high-pitched character. He finds that when the stimuli are presented simultaneously or with an interval approximating $.06\sigma$, a single fused sound was generally localized in the median plane. With greater temporal disjunction trained observers generally localize two distinct sounds in opposite quadrants until these dichotic sounds came to the oral axis at approximately 2σ . The relationship between these angular displacements and the temporal difference is approximately linear between $.06\sigma$ and $.126\sigma$ with much larger differences in temporal arrival usually above $9.48\sigma \pm .98$ which he considers the diotic threshold. Illusory movements of sound with as many as three phantom sounds were perceived.

In some further experiments (74) Trimble continued the use of the discrete impulse technique through spark-gaps using in general the same technique in regard to temporal disjunction in order to obtain further determinations of the fused phantom sound. Introspectively the observers found that it was experienced in terms of visual imagery of a more or less circular figure. This phantom sound comes to the maximal lateral position when the temporal difference is of the order of $.144\sigma$. It definitely divides into two sounds, one on either side (diotic) when the temporal difference ranges between

1.546 and 2.48 σ . Investigating these phenomena still further (75) he attacked that problem from five different directions: (1) as a function of intensive differences, (2) as a function of temporal differences, (3) with intensive differences opposed to temporal differences, (4) with temporal differences opposed to intensive differences, (5) with both factors varied in the same direction. Results indicate that intensive differences give more rapid displacement of localization than temporal differences. Temporal differences are not reducible in terms of his data to terms of intensive differences but they are not as effective as are phase differences in the region of low tones. He thinks that considering the entire range of hearing intensity may be the more effective factor in determining directional perception in auditory space. In another paper (72) he gives a very convenient summary of results thus far obtained by workers in the field, outlining the relative effects and limitations of the intensive, phase, and temporal factors. He comes to the conclusion that a compromise in terms of some form of difference-pattern is necessary at this stage of our investigations.

An ingenious device has been described by Bourdon (7) by means of which he demonstrated the relative importance of changes in intensity, phase, and time of arrival of two sounds led separately to the two ears. The apparatus consisted of a revolving disc. The factors were controlled by manipulation of tubes which led to the ears. He believes that intensity and time of arrival are both operative but questions the influence of phase. To the reviewer's mind, the evidence is not conclusive, although he must confess that he is not absolutely clear about all of the conditions of the experiment. Peterson (54) adds a contribution to the field by determining the frequency-range of the phase effect in sound localization. By using an ingenious heterodyning system he was able to produce pitches ranging from 10 $\sim$ to 16,384 $\sim$, while at the same time phase and intensity could be separately controlled. He found the effect of phase to be continuous between 10 $\sim$ and 8,192 $\sim$ and that it maintained approximately the relationship of Φ/Θ .

An extremely interesting set of conditions was introduced by Young (86) when he paralleled, to some extent, Stratton's experiment which inverted the retinal images. Young devised an apparatus which quite effectively transposed sounds that would normally strike the right ear to the left ear and *vice versa*. A number of detailed investigations were carried on with nine other observers but principally upon himself both under conditions of blindfolding

and with normal adjustments through the eyes and motor co-ordinations of the body. It turned out that without the conflict of eyes and ears localization was quite definitely reversed but with the eyes open there was a pronounced visual dominance; that is to say, although the sounds were being transposed abnormally to the opposite ear, proper motor adjustments were made to the outside sources of sound when the eyes were open. This leads the author to conclude that while reversal is possible the organism as a whole readjusts itself motorwise to the new situation. Again, therefore, we find that there is ultimate relegation of localized perceptions to the higher centers which are responsible for large neuro-muscular adjustments in terms of cortical sets. The physiological conditions of localization must, therefore, be sought in configurational changes of muscular tonus.

Auditory Sensations. The most recent attempt to systematize the auditory sensations in the tonal field has been made by the reviewer (61). The diagram called, for want of a better name, the tonal bell, had already been published in his text (63) and has been described also by Wheeler (84). Account was taken of the double qualitative aspect of tonal sensations variously designated but most often under the name of 'pitch' and 'quality', the latter of which the author has called 'tonality'. These distinctions were found to be supported by theorists and experimenters alike and by special investigations in the field of absolute pitch consciousness and of colored hearing. They were further validated by recent work on consonance. The basic idea is the spiral of Drobisch which allows pitch to rise continuously but in sweeps an octave apart on a line that is colored red in the laboratory model reaching below the threshold to the upper limit of hearing. The octaves are tied together vertically by blue lines indicating 'tonality'. Provision was further made for the attribute of volume in that the tonal bell has a flare at the bottom and somewhat of a dome at the top giving in cross-section an approximation to Titchener's tonal pencil. If further experimental work should throw this attribute into the discard the tonal bell would then be turned into a cylindrical shape but the spiral feature would remain. The author refers to this dimension as 'breadth'. Finally the spiral is given an accelerated or retarded rise in direct proportion to the number of distinct or discriminable pitch changes within each octave.

A critical review of the auditory attributes reveals to Gundlach (27) the present equivocal condition of at least two of the

four commonly ascribed characteristics. Pitch and intensity are fairly well recognized; brightness and volume hang in the balance. He gives specific directions for demonstrating each one as more or less separate variables. In a siren disc a given number of holes may be punched on concentric circles of different diameters so that on the outside there may be as much as ten times the distance between holes that there is on the inside, using the diameter of the hole as a unit. In this instance, tones from the outside are less voluminous and more intense than from the inside. Volume, however, seems to be the reciprocal of brightness and brightness may be explained by the presence of overtones. He was not able to confirm Rich's work on the liminal values for volume. Lau (37) believes that perceived intensity or loudness is not a direct function of physical intensity or the amplitude of the wave but that it is affected by the reflex influence of the visual percept of the field. Again we see that the sensory attributes are intimately tied up with associations of a perceptual nature in which adjacent cortical areas play a part.

Several additional facts in connection with absolute pitch consciousness have been reported. Gebhardt (25) notes the early appearance of this phenomenal capacity in a child of preschool age. At two years of age two different instruments were clearly distinguished and there was recognition as to whether the violin or piano were played alone or whether they were played together. At three a new street-car was recognized in terms of its different clang. Between three and four the notes on the piano were called off with few errors. At four years of age the child began to sing very accurately. Triepel (71) notes the persistence of a definite system in absolute pitch consciousness which was usually correct but always one tone too high. Experience showed that the father had trained the child on a French piano which was low in pitch.

Consonance, Dissonance, and Musical Intervals. A number of scientific studies continue the work in this interesting field. Wever (82) has published an investigation in two sections concerning beats and related phenomena resulting from two simultaneously sounding tones. He outlines three stages which partly overlap each other: (1) oscillations of intensity, (2) pulsations of pitch, (3) responses without intermittance. If the two sources are of equal intensity the clearest beats are produced but the regulating factors are the relative and absolute region of pitch, the relative and absolute intensity, and the criterion of judgment. He found a critical interval at which intertones and the two primaries are separately perceptible.

The lower threshold for the beating phenomenon is largely determined by the observer's patience. The author has perceived beats as low as one beat in two minutes. In the second section the beats of mistuned consonances are investigated. The author deals separately with three theories that apply to complex tones (clangs) as follows: (1) overtone theory, (2) transformation theory, and (3) resultant displacement theory. The author rules out the overtone theory as an incomplete explanation. The second theory assumes that a series of partials is produced within the auditory mechanism as the result of a strong stimulation by a pure tone. This the author admits, not only as regards overtones but combination-tones as well, even when these tones are not directly perceived. The resultant displacement theory stipulates that the pulsations which we hear as beats correspond to a wave obtained by adding the amplitudes at all points, the beats themselves being represented by regions of maximal amplitude in the resultant wave. This theory proves to be inadmissible, however, partly because what may be visual analysis when the conventional curves are drawn has still to be proved as auditory analysis in terms of a complexly presented wave. Another reason for dismissing it is the general acceptance of Ohm's law which would make it difficult to account for peripheral analysis in one breath and peripheral interaction in another. The author concludes this study with a statement regarding auditory theory and votes definitely in favor of the Helmholtzian resonance-place type of theory. The reviewer heartily agrees with him on the basis of the outstanding fact that while other theories have explained a few specific phenomena the Helmholtzian theory has accounted for a larger group of related facts. While there is a general drift towards greater participation of the central areas due to the perceptual character of most of our experimental results the operation of the peripheral mechanism is most satisfactorily explained by some form of modified sympathetic resonance in connection with the basilar membrane.

Hauge (30) made a study of beats with respect to the alleged phi-phenomenon found in vision. The problem involved the relations between the beating complex and the intensity of the two generating tones. He investigated the effect on the beating intertone while the intensity of one primary was decreased at the same rate as the intensity of the other was being increased. On the basis of observations by four individuals the writer concludes that through his technique the phi-phenomenon is not applicable to the concept of beats. No perception of auditory movement was reported. Movement

as such when it did occur was merely inferred. An intertone was heard when the respective intensities of the generating tones were unequal, in which case it approximated the pitch of the more intense generating tone. When the intensities of the two generating tones were equal or nearly so, two intertones were heard, each one nearly equal in pitch to that of its respective generating tone.

After distinguishing between *interval quality*, or the phenomenal characteristic of intervals that gains recognition regardless of fixed frequencies but in terms of a fixed ratio of frequencies involved, and *distance*, which indicates the absolute tonal separation, Pratt (55) continued his former studies, reviewed in a previous summary (62) by bisecting intervals larger than an octave. Below the octave bisection follows the geometrical mean or an equal division of the ratios of frequency fairly uniformly; above the octave, however, this is not the case. Four intervals were used, one slightly larger than the major ninth, one about half way between the eleventh and the diminished twelfth, one the just twelfth, and one slightly smaller than the minor fourteenth. Pratt believes that the difference between these and his former results lies in the difference of the observer's attitude. If he judges in terms of a *musical interval*, the midpoint still coincides approximately with the geometrical mean but if *distance* is the criterion this point will move along toward the arithmetical mean. Concentrating then on *distance* in another study (57) he asked his observers to compare a variable interval succeeding a constant interval in terms of *distance*. During one-half of the series the temporal order of the tones forming the respective intervals was reversed. This study verifies his former conclusion in connection with the usual change in attitude when the interval exceeds an octave. In the region of an octave and a half the apparent size of the interval is increased by about as much as a minor second. The reviewer wonders whether the conditions of both experiments did not tend to move the observers away from the natural musical setting surrounding interval relationships when he admits that they judged fifths differently in different regions of the tonal register. In the former experiment the traditional *Aufgabe* persisted within the region of its most frequent operation; in the latter the requirements of the experimental series were more rigorously lived up to. The author is quite right, however, in making the distinction which in the reviewer's opinion is an analog to the situation which makes *c* and *c'* more alike than *c* and *d*.

In a third study (56) Pratt makes a very useful diagnosis of

quarter-tone music. He thinks that size of intervals has little to do with pitch discrimination since a change in size can not be detected if one of the tones is slightly raised in pitch but well above the normal threshold for pitch discrimination for that region. In the middle register a change of 20 cents is noticeable but gives only 50 per cent right judgments. An unqualified 100 per cent correct answers can be obtained only when the change is 50 cents. The reviewer is reminded of that fact that in violin playing and probably in singing the minor third is flatted and the leading tone sharpened not to play "in pure intonation" as is frequently erroneously stated, but to emphasize the given effect without losing sight of the range within which the interval would still be recognized for what it is. The author sums up his analysis by quite correctly insisting that the musical significance of an interval is influenced by other factors than perceptibility. The imaginal features of the percept have at least equal rights with the sensory constituents.

Densmore (16) revives the question as to the reason for the deviations from the semitone scale found in Indian music. While the Indians are more susceptible to the significance of sound of many types that are imperceptible to us, certainly their pitch discrimination and probably their auditory acuity is not greater than ours. After analyzing some 1,700 phonographic records of their music she concludes that the reason lies in the lack of a conventionalized system of music and an emphasis, on the other hand, on individual expression.

Metfessel's monograph (41) on negro music makes claims in the direction of a new and vigorous attack on certain psychophysical relationships in primitive vocal and facial expression. By combining ingenious devices and by using a new musical notation, he was able to record both the facial expressions and the intonations of the singer and later to analyze the effects recorded. The monograph is replete with detailed accounts of a variety of songs as sung by many different singers under varied conditions. It treats of the emotional expression, the use of the vibrato, the specific analysis of many songs in terms of an elementary, objective, and unbiased attack on a type of music which is so unlike our own that any subjective appraisal and analysis allows the vitiating factor of our perverted perception to enter and thus to color the conclusions drawn. The study deserves a separate and longer review.

There continues to be an active interest in the more fundamental questions surrounding consonance. Metfessel, as we noted in a previous review (60), re-emphasized the close analogy between har-

mony and melody that Bingham and Stumpf, among others, had discussed in the historical literature. Now Kollarits (33) from another angle suggests that fusion and assimilation can not be sharply separated. The running course of a melody approaches the harmony of a struck chord, especially if it is struck *arpeggio*. There is simply a fusion of a whole series of images in a pattern in the melody, whereas in the chord there is a fusion of two or more whole images. This theory, as was Bingham's years ago, is motor in type. One of the most thoroughgoing and scholarly studies recently made is contributed by Guernsey (26) who took as his problem an attack on and an evaluation of various criteria which have recently been discussed in connection with consonance: fusion, smoothness and affective response. Both musical and unmusical observers were used and control apparatus was devised to give both pure tonal and complex tonal stimuli. He concludes that consonance, within the strict interpretation of its musical meaning, can not be judged adequately in terms either of fusion or smoothness. Tonal fusion becomes then not a perceptual but a sensorial phenomenon absolutely different from its æsthetic or affective experience. Smoothness is likewise inadequate as a single criterion because of the divergence in connotation in the mind of the listener. The author believes that pleasantness and unpleasantness in the last analysis are the most legitimate criteria to use in the musical setting in which consonances and dissonances find themselves. The criteria which have been traditionally used in experimentation are influenced by too many factors, like intensity, clang, and temporal sequence, to be scientifically accurate. He closes his paper with interesting applications to some actual musical compositions and theoretical situations. Incidentally the minor second stood out as occupying the lowest position of consonance in all series.

Guthrie and Morrill (28) confirmed Brues' experiment on the fusion of nonmusical instruments by the use of a technique different from that reviewed in our previous summary (60). Using Stern variators 44 intervals were produced ranging from perfect unison to an interval beyond the fifth. The observers ranked each pair as pleasant or unpleasant and again in terms of consonance and dissonance. All 372 observers were taken from an elementary class in psychology and knew the terms presumably only in a general way. The curves for pleasantness and consonance show a high agreement. All of these studies would raise the question whether the affective response should be left out of account in the test of consonance as applied to musical ability.

An unprejudiced investigation was conducted by D. L. Larson (35) in connection with the Seashore consonance test answering at the same time several objections made by Heinlein. The main problem involved a strict adherence to the instructions which accompany the test requiring the observer to judge cognitively instead of affectively but admitting at the same time that the instruction as presented in the manual accompanying the test is probably not as emphatic in its direction as it might well be. The results were statistically treated in terms of correlations and the outcome showed that the procedure of comparisons of paired intervals which was criticised by Heinlein was adequate for testing consonance and did not lead to harmonic progressions which perceptibly disturbed the scores. If the instructions are literally followed the consonance test proved reliable either as a group or as an individual test in terms of retesting. With affective judgments eliminated under the controlled conditions of the experiment, observers with pronounced musical ability scored relatively higher than those without such musical capacity. Some of the criticisms are admitted in the discussion that follows.

Pitch Discriminations and Auditory Acuity. It is now fairly well established that work on pitch discrimination must go hand in hand with the investigation of auditory acuity. In view of an increasing number of investigations it were folly to test only one or the other of these attributes without either keeping one of them constant or else, as is more frequently done, investigating the one factor in terms of a wide range of the other. The common practice seems to be to treat one functionally in terms of the other and to plot both graphically in terms of ordinates and abscissæ. Bunch is continuing his work in audiometry with abnormal cases using groups of normal subjects as a check. In one of his studies (10) he points out that the traditional explanation for the loss of acuity for tones near the upper limit has been interpreted as a lesion of the auditory nerve. By means of ordinary standardized techniques such as are used with the Western Electric 1A audiometer the upper limit for hearing high tones at the intensities available in the apparatus is considerably below limits established by more intensively given tones. One case yielded audibility at 5,760~ on the Western Electric 1A audiometer and 11,000~ by means of the monochord (Struycken modification). Through bone conduction this was raised to 23,000~ in the left ear. Many cases of so-called deafness, therefore, turned out to be a loss in sensitivity. Bunch (9) also investigated 353 cases at various age limits from twenty to sixty years where at any stage there was no

subjective indication of deafness. A wide range of tones was also used (32 ~ to 16,384 ~). At 1,024 ~ and below he found that the scattering of results was quite uniform with considerable broadening for higher tones and a most marked effect at 8,192 ~. With reference to your various ages at the decades studied he found little difference with age under 512 ~ but a decreasing sensitivity in the higher octaves. One curious result which is hard to explain was a very definite determination that for the sixty-year-old group the average threshold for 512 ~ was lower than in all other age groups. Why people of this age should hear better in this middle register than younger individuals requires further investigation and explanation. He has also worked with a case where the entire right cerebral hemisphere was removed (8). When the patient had completely recovered from this excision no disturbance occurred in the perception of pure tones or spoken words. This is another instance following the work of Franz, Lashley and Cameron which points to the tremendous amount of reserve function that is available in case of an emergency and the necessity for emphasizing the complete integration of function rather than specific localization.

Corbeille and Baldes have made two studies (13, 14) on responses to acoustic stimuli in intact and decerebrate animals. In one case with cats, dogs, frogs and rabbits respiratory changes were noted and in the other cardiac responses in rabbits. In the latter case the Einthoven galvanometer was used. An electric oscillator and loud speaker produced the sound and records were taken only after the animals were emotionally adjusted to it. These studies add material to an already long list of researches gathered by Diserens (18) in a book which has been separately reviewed in the *PSYCHOLOGICAL BULLETIN*. In the case of frogs prolonged tones decreased the respiratory rate whereas short rapid tones increased the rate. While analysis was made of the effects at the onset of the stimulus, during the application of the stimulus, and during the after-effect of the stimulus, only the second and last of these situations yielded consistent results. During stimulation the rate of breathing was decreased for both types of stimuli whereas in the after-period the rate was increased often considerably above the normal. The decerebrate frogs and rabbits responded in the same way as those which were kept intact as far as breathing was concerned but the intact rabbits showed decreased heart-rate while the decerebrate rabbits showed no effect. In a few cases the heart-rate was increased in the decerebrate animals.

Several papers have attempted to evaluate tests of auditory acuity in terms of the larger hygienic program involved. Barwell (2) seems to think that the various tests have a place in the field of nerve-deafness, middle ear deafness and otosclerosis. Various precautions for their use in these connections are stated. Bevie (3) enlarges this program for the prevention and control of defective hearing. Four types of activity in this field are outlined: (1) the study of the hereditary basis of some of the types, (2) the training of obstetricians on the detrimental effect of prolonged labor on the part of the mother and the necessity for immediate cleansing of passages together with the elevation of the head of the new-born, (3) the education of workers and employers in matters of industrial hygiene, and (4) the protection from the effects of detonation in the army and navy. Some studies have also indicated the serious practical problem of deafness in our population. Morton (50) estimates that there are three million public school children in this country who are afflicted with incipient deafness. About four-fifths of these can be kept from becoming deafened adults by giving them proper vocational guidance. Lemcke (38) shows that deafness is more serious than restriction of visual acuity or other visual defects since it often involves a moral deterioration. By separately housing children afflicted with this malady disturbing inferiority complexes may be avoided.

Carrying on similar experiments on frogs Corbeille (11) found again that sounds from 100~ to 8,000~ caused respiratory retardation for the duration of the stimulus but short repeated sounds caused acceleration. The same results followed decerebration above the optic lobe in another set of frogs, hence the conclusion that in this group of animals the cortex is not directly involved in this response. Further experiments by the same author (12) on the cardiac responses of rabbits to sound showed an invariable effect in the direction of retardation of the heart-beat. If stimulation lasted from 15 seconds to 1 minute 5 seconds, the rate returned to normal. In 60 per cent of the cases decerebration destroyed the effect, indicating perhaps a bulbar reflex pathway.

Upton (78) furnishes us with a useful summary of the recent investigations on auditory sensitivity among the higher vertebrates. The general impression obtains that many of the mammals hear noises but not tones as such. The author criticizes the results, however, in terms of the procedures employed since the tones involved played so small a part in the total situation to which the animal responded. He therefore discards the procedure in favor of a con-

ditioned breathing response used with four guinea pigs. Tones of 600~ did not at first modify the breathing rate, but when later conditioned with an electric shock there was a definite breathing response during stimulation and a return to normal after the cessation of the stimulus. That this response was to a specific tone was proved by testing with one of 1,000~, when no response was elicited.

There has been unusual activity again in the field of auditory sensitivity occasioned, as was before noted, by rapid advances in electrical apparatus largely of the oscillating tube type. Kellogg (32) using an oscillator devised by Halverson and noted in our last review (60) started out to establish the auditory intensive threshold in terms of electrical units. Through suitable equations the scale readings in centimeters were converted into millivolts. The premises underlying this conversion were examined and found to be approximately true. The intensity of tone appears to be linearly related to the amperage required to produce it. Threshold values are obtained both in millivolts and in per cent values of standard stimulus. Riesz (58) employed currents from two oscillating circuits which were alternately and automatically switched to the observer's telephones about three times a second. When fluctuations in loudness were reported the result was noted. Weber's law held provided the intensity was not less than 10^6 times the minimal audible intensity but varied slightly from tone to tone (35~ to 10,000~). The variations lie between .05 and .15. Wever and Truman (83) tested out the course of the auditory threshold in the presence of a tonal background, in order to parallel the situation which usually obtains in everyday life where sounds have to be distinguished not in absolute silence but in the presence of other sounds. In general, they found that the threshold is at first very high in the presence of the ground-tone as compared with conditions of quiet but with repetition its value is reduced until it approaches normal hearing. This level of sensitivity proceeds at an ever decreasing rate until after about two minutes it has practically ceased even up to nine minutes of stimulation, but it is never quite as low as when the background is entirely quiet. Some attempt is made to invoke the *Gestalt* theory in terms of figure and ground suggesting a change in the observer's total experience making it appear somewhat analogous to visual phenomena.

Onoshima (52) contributes an interesting study which parallels in man some of the work that has been done with the lower animals. According to the *Gestalt* theory, he proves that the judgment of an

equal pair of comparison tones on the criterion of intensity depends upon the rhythmical *Gestalt* of the whole series. A shift in the rhythmical pattern will very much disturb the judgment of groups of tones. Although it is not directly stated, some configuration of the same sort must be involved in the study of Kreidl and Gatscher (33a) who dispute Stumpf's statement that a tone sounds higher in pitch to the right than to the left ear. In their work an individual difference appeared which could perhaps be explained by a difference in cortical set or *Aufgabe*, in that in some individuals the second excitation was generally heard higher in the right ear, while in others it was heard higher in the left ear.

Apparatus. Hartmann and Trolle (29) have developed an air jet whose velocity is higher than that of the sound which serves as the fundamental. They propose it as a new acoustic generator and offer an historical and theoretical discussion together with charts, drawings and photographs. Dimmick (17) also suggests some new auditory apparatus which is built on the principle of the oscillating circuit and produces relatively pure tones over a wider range than the full set of Stern variators by the method of beat-frequency. He was able to obtain tones from the lowest audible tone to one near the upper limit ($20 \sim$ to $9,000 \sim$) but tones even lower ($4 \sim$ to $5 \sim$) were heard by means of a telephone. The tone is proved to be sinusoidal. Above $5,000 \sim$, however, harmonics appear.

Miles (49) claims that if weights are set at unequal distances on the respective prongs of tuning forks, the prongs are likely to vibrate at the same frequency but with different amplitudes, thus influencing the total duration of the tone produced. Photographs and a bibliography are included. Metfessel and Tiffin (46) give a brief description of their new Phono-Projectoscope. This very useful and much needed improvement over the Koenig rotating mirror, consists essentially of an horizontally rotating drum with six projecting and reflecting plates. By this means the optical lever is spread out in the usual fashion over a much broader field and a clear demonstration is afforded of the analysis of the form of sound waves. A photographic method of measuring pitch is described in some detail by Metfessel (42, 43) who utilizes the stroboscopic principle to do away with the tedious and expensive method of measuring wave-lengths after they have been recorded. The photographic film serves as the graphic paper and the mode of recording thus shows the deviation in wave-length or frequency. An example is given illustrating a graph of Galli-Curci's voice on two short notes. Fritz (23)

suggests an apparatus for measuring the reaction time of white rats to noise. The experimental animal is placed in a basket which is conical in shape and has an aluminum disc as its base. This is suspended in such a way by a rubber band that movements will be registered by means of electric contacts which alternately make and break the signal current.

The bound volumes of the *International Critical Tables* furnish an exceptional reference library for authentic information in acoustics. The following instances give an adequate sampling of what each volume contains. Watson (80), for example, tabulates the critical values in connection with sound generators. He gives the data concerning the relation of the frequency of tuning forks to temperature and to amplitude of vibration. In the same way he discusses the acoustic amplitude and efficiency of telephone receivers in relation to the frequency of the current. Devices are also suggested for stabilizing the frequency of oscillating currents. Sabine (64) discusses the transmission, reflection, reverberation, and absorption of sound. The values are given for transmission through tubes, horns, and wave-filters. Similarly tables show the effect of various types of septa bounded by air, such values being given for quilts, wood, glass, and steel. Acoustic absorption is likewise dealt with. Fletcher (20) gives detailed figures on the masking effect of sound and on the sensitivity of the human ear. Foley (21) notes the velocity of sound for different media. In free air and in illuminating gas this velocity is apparently independent of pitch but not independent of intensity especially at high values of intensity.

Psychology of Music. Interest still centers in many branches of the psychology of music. There is much discussion of the adequacy of musical tests as an index to musical talent. Heinlein (31) wonders whether the degree of difficulty in the test for tonal memory can be measured by the length of the span of tones to be remembered. Talented individuals think in terms of melodic patterns and the tendency is pronounced in these individuals to avoid the instructions which require counting. Melody involves a motion of pitch that may be either acute-oblique, horizontal or grave-oblique. Cutting across these motions are the factors of the rhythmic pattern and the place of pitch in the tonal register. He does not think that the tonal contour given in the test can be avoided. In a similar vein Lowery (39) notes that listening to music is like reading. The tones become the letters of the word or the words of a phrase and need not, therefore, be individually recognized. One may be able to hum

the melody correctly without specific attention to the notes by following the meaning of the passage or pattern. He does not believe that the Seashore test stresses sufficiently the musical form as a whole. And so he selected distinctly musical phrases in his investigation with 130 children, twelve to fourteen years old. The initial phrase was given three times and was followed by examples somewhat modified by transposition, embellishment, diminution and augmentation. He found that scores on this test were significant and that general intelligence played a distinct part in the recognition of memory and cadence but showed a zero correlation with the scores for the test on phrasing.

Fracker and Howard (22) have investigated the correlation between intelligence and musical talent among university students, 230 of whom were tested at the University of Arkansas. Intelligence scores were derived from the Otis Self-Administering tests and the Army Alpha test. The highest correlation ($.32 \pm .039$) was between pitch and I.Q. Some of the previous scores had been considerably higher but the authors agree that general intelligence favors the securing of a good record especially in the first test and that of necessity no high correlation between musical ability and intelligence need be expected. Conversely, however, outstanding musicians are usually persons of great intellectual achievement.

Stanton (69) has published in monograph form an extended evaluation of the Seashore tests in connection with their practical application over a term of years at the Eastman School of Music. This monograph deserves more extended review. She has been able to justify the giving of these tests for the purpose of prognosis and of classification within various divisions of the school. That these classifications were satisfactory was borne out by the mortality of the low group on the one hand and the predicted achievement of the high scorers on the other. The forms used for rating and case-records together with other criteria are added. She also (68) makes a report in the Seashore Commemorative Volume in which she shows that in various studies the teachers' talent ratings were fairly well matched by the test scores before this procedure was used for admission to the Eastman School of Music. Both of these studies constitute, so far, the outstanding trial in practical school work which these tests have received.

Three of the Seashore tests have been tried out with preschool children by McGinnis (40): intensity, pitch, and consonance. There are naturally several difficulties to be faced: the length of the task,

the comprehension of the terms used in judging, and the uninteresting nature of the material; but the writer believes that they are adaptable with some slight modifications in the direction of shortening the series and increasing the interval between judgments. The correlations are found to be comparable to those obtained in other investigations, but, of course, there are more omitted responses. Curiously enough in view of other trials, the consonance test was found to be fairly reliable. W. S. Larson (36) used the tests for purposes of prediction of success in public school instrumental classes, especially with orchestras. Detailed analyses of scores made in the several tests are given with the general result that tests for pitch, rhythm, and possibly intensity, give slight indication of probable success in beginning classes. Tests for time, consonance and tonal memory are apparently of little value here. Tests for pitch, intensity, consonance and rhythm are significant for those who continued into the junior high school orchestra; all except those for intensity are significant for those who qualified for the junior high school advanced and preparatory orchestras; and all six tests without reservation were of still greater significance in selecting the membership of the high school advanced orchestra. Nielsen (51) concentrated on the problem of training and the predictive value of the rhythm test. Ten poor and ten good students from the School of Music at the University of Iowa who were used as observers showed that the test correlates significantly with musical ability and performance, and that motor rhythm can be slightly improved in the early period of training. These statements, however, do not hold, he finds, in connection with the ability to tap out a rhythm.

Gaw (24) offers five contributions to the *Seashore Commemorative Volume*: a sight singing test, a modified time discrimination test, a modified form of tonal memory test, some fifth grade norms obtained by means of the Seashore test, and some preliminary studies on pitch discrimination at Mills College. While these tests, for the most part, were not done in sufficient number or under rigorously controlled conditions they do offer suggestions by way of adapting tests to situations involving musical ability.

A number of studies have dealt with the question of the vocal vibrato. Metfessel (45), who is an outstanding authority in this field, has analyzed some new examples of vibrato in accordance with wave-frequency, amplitude, form, and periodicity. After studying some 615 different cycles he finds that the extent of the vibrato in frequency is about one-half step ranging from one-fifth to a whole

tone and that the periodicity is 7 cycles per second with greater uniformity in rate than in extent. Four types are distinguished with respect to relative length of crest and trough. A general account of the vibrato as found in connection with such celebrated voices as Caruso, Galli-Curci, Chaliapin, McCormack and Homer is described by him (44) in an article which describes the part of the vibrato in producing the æsthetic effect in music. He contrasts the analytical attitude of the laboratory and the effect of fusion produced by the vibrato at a concert. The former makes science; the latter makes for enjoyment. In a brief note William (85) summarizes previous investigations on the extent of the vibrato as carried on in the University of Iowa laboratory. He differs with Metfessel in the interpretation of the discrepancy between the results obtained by Schoen and other investigators in regard to the pitch-range of the vibrato. He does not believe that the discrepancy is due to the presence of over-tones but to the rate at which the phonograph record was run and the tendency of the tonoscope to respond to multiples of the original rate. Wagner (79) has published a thesis that shows how well the vocal vibrato may be controlled and developed through ingenious breathing devices used with a wide range of observers. Through the use of models from artists' tones he was able to cultivate the vibrato where none before existed and to refine the control of the rate of vibrato through the influence of rhythm, and the extent of vibrato through rhythm and controlled respiration. Numerous graphs showing the final accomplishment of his observers, among whom were both children and adults, are presented.

Stevens and Miles (70) have worked out the question of attacking the tone during singing. Ten men furnishing 360 records proved that the human voice raises its pitch from 1 per cent to 3 per cent within the first one-fifth of a second of singing. Later the rise is more gradual and by the end of one-half second there may be even a slight tendency to lower the pitch. Humming is less certain as a mode of attack than is open singing and singing on a full breath seems also to improve the attack. Previous singing at about the same level of pitch is the best guarantee of a successful attack. The study is unique in that the Dodge microscopic recorder was used. Root (59) has made an extensive analysis of pitch-patterns and tonal movements in the speaking voice. He finds that there is much similarity between the melodic recording of song and the cadence of speech. A number of standard addresses were analyzed by fourteen trained observers who were ignorant, however, of the purpose of the study. The results

showed a high degree of consistency among the reports. The method of phonophotography was also resorted to in another section of the investigation. In both sets of records individual differences in the evenness of pitch appeared, but, on the whole, there were manifest tendencies toward definite pitches and characteristic pitch-patterns which were analyzed in detail. Musical intervals in both sets of records were also approximated. Similarly the syllabic pitch-patterns and transitions from syllable to syllable were scrutinized in terms of seven criteria which are given. Metzger (47) has entered the controversy concerning the mode of vibration of the vocal cords in a thorough-going study and with very careful technique. A part of the technique involved swallowing small X-ray plates so that a photograph might be obtained from the front of the throat through the vocal cords which as a result of his study he prefers to call voice-lips. He finds no alternate action of these voice-lips but what he calls a 'cushioned action.' The pharynx does not change to the next octave above or below but the change of voice both from one register to another and during adolescence in boys is due to an interaction between the voice-lips and the bronchial resonance. A comprehensive survey of both speech and hearing is now available through Fletcher's (19) new book which is worthy of an extended review in the *PSYCHOLOGICAL BULLETIN*. He summarizes in a completely competent way the outstanding facts with regard to the recognition of sound and the basic principles underlying hearing. His numerous and notable contributions to this field ought to guarantee him an attentive audience.

In a short study Sherman (67) reports the effect of the emotions of surprise, fear-pain, sorrow, and anger-hate as expressed in terms of a single vowel repeated five times. The observers were to tell what emotions the singer intended to convey. Intensity, duration, and pitch were kept constant and visual cues were eliminated. While the investigation was sketchy in character, sorrow and anger-hate were rather well detected. Fear-pain was always conveyed and surprise showed little effect. The study is more indicative than authoritative. Sapir (65) gives us a very suggestive study in phonetic symbolism. He distinguishes two forms of meaning found in words, the dissociated or referential type found in the transition from 'boy' to 'man' and the expressive type conveyed by a change in intonation or of vowel quality. Examples of the second are the change from the interrogative to a rhetorical question or the change from 'tiny' to 'teeny.' He demonstrates that there is a wide

individual difference in responsiveness to such sound contrasts and outlines a number of various significant results obtained with the use of disyllabic nonsense words. Observations show that auditory and kinæsthetic factors are involved in the meaning. Schoen (66) summarizes the æsthetic attitude in music and the literature that has accumulated. He analyzes the question of the beautiful and the associations which come to the well-informed listener. It is epitomized by the statement, "the beautiful in music lies in 'listening to music,' and not in 'hearing music'." The essential fact is that to get the most adequate enjoyment out of music one must try to understand the basic musical form in terms of the meaning which it expresses. Music, he says, furnishes "the ideal conditions for beauty as a self-sufficient, complete in itself, intrinsic experience."

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TOUCH AND KINESTHESIS

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A large number of studies have appeared in the field of touch since the publication of the last summary of the literature on this topic.¹ The number of studies allotted to the temperature sense or to the problem of establishing the specific end-organs of temperature is perhaps greater than that falling to any other aspect of touch. The relatively unexplored field of the sense of vibration claims the attention of a noteworthy group of investigators. Attention is called to several important investigations which indicate specific facts in regard to the nature of the sensory discharge of impulses in cutaneous end-organs. While the experimental results of these recent studies in many instances support the existing theories, in others they flatly contradict certain of the theories of touch. Owing to lack of agreement between experimental fact and theory, suggestions in the way of new theories are submitted.

In contrast with the large number of studies on touch, there are relatively few contributions to our knowledge of the kinesthetic sense. With few exceptions, these studies have only an indirect bearing upon kinesthesia, such as its rôle in a learning situation or in the perception of space. There are several articles which are concerned with clinical problems, but with few exceptions, both as regards the cutaneous and kinesthetic articles, the conclusions and generalizations advanced are supported by carefully prosecuted experimental work.

Receptors. A number of articles deal with the problem of cutaneous receptors, and several present results as to their depth in the skin. The majority of these investigations are limited specifically to the attempt to locate, if possible, the receptors of the temperature sense. The most significant net result is the failure of these investigators to reach any noteworthy agreement in regard to the specific end-organ for either of the temperature qualities.

Dallenbach (17) indicates the two methods, direct and indirect,

¹ JOHN T. METCALF, Cutaneous and Kinesthetic Senses. *Psychol. Bull.*, 1928, 25, 569-581.

which have been used to locate the end-organs of temperature. The direct method, by which tissue is extirpated and examined, has given negative results. The indirect method, used first by Donaldson and by Goldscheider, has led to "positive correlations which, though tentatively advanced, have been dogmatically accepted." The same author (16) finds that the punctiform method of localization of temperature spots yields a significantly higher degree of correspondence in repeated surveys of the same area than the traditionally used moving method. Twelve sources of error in the latter method are designated. He finds that, by the use of the punctiform method, the warm spots are as numerous as the cold spots. Histological examination fails to reveal end-bulbs of Krause and Ruffini cylinders in the skin beneath these spots.

Pendleton (75) uses the "best histological technique available" and fails to find Golgi-Mazzoni end-bulbs in the hairy parts of the skin at which cold spots had been localized. These terminals are found, however, in the palm of the hand. By measuring the limen of the same cold spot before and after removal of a layer of skin of known thickness, a method is contrived by which it is estimated that the cold receptor is located in the stratum of Malpighi. Free nerve endings and the hederiform nerve endings of Ranvier, the latter of which are found in warm blooded animals only, are found in this layer. The latter are designated as possible receptors of cold.

Strughold (92) explores the sclera and cornea for sensitivity to cold, and presents evidence in support of the view that the end-bulb of Krause functions as the cold receptor. He finds the threshold of cold to be lower at points beneath which the end-bulbs of Krause are more superficially situated, and the density of cold spots to be greater in parts under which the organs of Krause are more closely collocated. Gilbert (41) reaches a different conclusion as a result of microscopic examination of portions of skin, taken from seven different parts of the body. Pacini corpuscles are found, especially in the skin from the sole of the foot, but end-bulbs of Krause and Ruffini cylinders are not found anywhere. It is assumed that the technique which reveals the Vater-Pacini organ should also expose any other encapsulated end-organ.

Adrian and Umrat (4) secure records of the discharge of impulses which follow the excitation of the Vater-Pacini corpuscle under the flexor tendon of the cat's toe. They find that these organs respond to mechanical, but not to thermal stimulation. The records of the discharge of impulses resemble those obtained in the living

animal when pressure is applied to the toe-pad. The Vater-Pacini corpuscles are accepted as the receptors of pressure applied to the skin and of changes in position at the joints.

Having determined the latent time of the sensation of warmth, Hahn, Boshamer and Goldscheider (50) estimate that the warm receptors are not more than .1 mm., and most probably only .07 mm., below the skin surface. The intraepithelial nerve endings are indicated as the probable receptor of temperature. Pütter (80) also attempts to determine the depth of the end-organ of temperature. He finds that the size of the difference limen of warm varies inversely with the duration of the stimulus up to .8 to 1 sec., beyond which there is no change as a function of time. In dividing the amount of temperature change by the duration of the stimulus, a constant is found, from which it is calculated that the warm end-organ is in the stratum corneum at a depth between the limits of .012 and .032 mm.

Temperature sense. The contributions to the temperature sense throw light upon various conditions of thermal stimulation. A number of studies deal with certain phases of the problem of thermal adaptation. Hahn (47) analyzes adaptation (*Umstimmung*) into two phases: *adaptation to stimulation* (*Abstimmung*), during which sensitivity is reduced, and *recovery from adaptation* (*Anstimmung*), during which sensitivity returns to the normal status. At low intensities, adaptation to stimulation reaches complete insensitivity, but at higher intensities there is reduction only to a constant level of sensation. The more intense the thermal stimulus, the more rapidly the phase of recovery from adaptation takes place. Using both external and internal (diathermy) stimuli, François and Piéron (25) demonstrate that thermal adaptation consists of a state of equilibrium in a zone within which thermal sensitivity disappears.

Von Frey (27) says Weber's theory of temperature is apparently contradicted by the fact that warm is not experienced immediately upon removal of a cold stimulus. He points out that this will occur, however, if precautions are taken to make sure that warm organs exist in the region of stimulation and if a Holm guard is used to insure the reduction of temperature does not extend beyond the area covered by the cold stimulus. Hahn (45, 46) finds that the two hands experience no difference in temperature sensation when placed in a common bath immediately after each has been separately adapted to very different temperatures. He shows that the temperature sensation is entirely independent of the rate and degree of temperature

change. He concludes that the adequate stimulus for the temperature sense consists, not in the matter of change in temperature as Weber believed, but in the absolute temperature of the sense organ itself. Experiments in which they find that adaptation to any temperature above or below physiological zero alters sensitivity uniformly throughout the thermal range between physiological zero and the temperature of adaptation prompt Hahn, Goldscheider and Bruch (49) to formulate the concept, *zone of adaptation* (Adaptationsbreite). They derive another concept, the *law of constant sum*, from their discovery that the sum of any temperature of adaptation and the subsequently determined just noticeable difference in temperature represents a constant at all points in the range of temperatures. Thus, after adaptation to 33 degrees, the difference limen is .9; to 30 degrees, about 4; and to 10 degrees, about 24 degrees C. Piéron (77) shows that, after a period of adaptation, the threshold of temperature is altered, while that of the burning experience which is associated with thermal stimulation is unchanged.

Hahn and Lueg (51) and Hahn, Boshamer and Goldscheider (50) claim to have demonstrated that cold and warm sensations may be elicited simultaneously in the same region without fusing into heat. The former study casts doubt upon the existence of simultaneous contrast in temperature experiences.

Bishop (9) has designed an improved heat grill which allows simultaneous application of warm and cold, or rapid alternation from one temperature to the other. Messerle (71) describes a temperature apparatus which keeps the water contained in it at constant temperature for periods of time sufficiently long to make clinical examination of the temperature sensitivity of patients.

Burnet and Dallenbach (11) and Gritman and Dallenbach (44) present different combinations of warm and cold water through a pair of heat grills, upon each of which the subject places a forearm. The subject is required to make a comparative judgment as to which is hotter. It is found that the number of hotter judgments varies with the size of the interval between warm and cold stimuli when both intervals in any pair are referred to the same standard, but not when referred to different standards. The stimulus value of the different temperatures is found to vary with the distance from physiological zero. An equation for heat intensity is derived in which the warm component is weighted by the constant three.

Straus and von Versen (91) show that recovery of thermal sensitivity after the skin has been anesthetized by the application of

cocaine varies with intensity of stimulation. The results for warm are more conclusive than those for cold.

Pressure. Hulin (53) describes an electromagnetic esthesiometer which eliminates certain errors of other electrically operated instruments of the kind. Gatti and Dodge (34) devise a new pressure gauge, which controls precisely the rate and period of application of a stimulus. By stimulating a single pressure spot, they find that the just noticeable difference does not vary with the intensity of the stimulus but is an absolute constant. In another study, Gatti and Dodge (35) use a refined technique by which changes in the deformation of the skin under varying degrees of intensive stimulation may be observed. They reach the conclusion that physical pressure (gr./mm.) is a less satisfactory measure of the adequate pressure stimulus than their new formula which expresses the adequate stimulus in terms of the depth of deformation divided by the cube root of the total weight squared.

Fischer and Grundig (24) determine that the skin is normally more sensitive to tangential than to direct or vertical pressure. They glue thin metal discs on the skin and apply pressure vertically or tangentially by means of hooks which are fastened in the center or edge of the disc. The threshold for tangential pressure is smaller for all dimensions of disc used than that for direct pressure. Believing that movement of the skin is responsible for this difference, they determine the tangential and direct pressure thresholds on the knee-cap, where the skin may be moved readily, with the leg in the extended and in the bent or flexed position. The threshold of tangential pressure is found to be much lower in the extended position, but in the flexed position, where lateral displacement of the skin is greatly reduced if not entirely eliminated, the two thresholds are practically the same. The threshold of the skin when under tension is found to be doubled for the direct, and tripled for the tangential mode of application. Von Frey and Strughold (28) find that the threshold of pressure after anesthetizing the skin is greatly increased. The thresholds of pull and pressure after anesthetizing the skin are the same. Owing to the fact that pull is not believed to stimulate the deep tissues, the authors believe they have confirmed Weber's view that the skin is unitary in its function. Previously Lombard (66) asserted that tickle and touch are excited at different points on the skin, but he now concedes that at least certain spots on the skin give rise both to tickle and to touch. Eidelberg (22) reports that sensitivity to wetness and dryness differs in various parts of the skin,

being most acute on the tongue, then on the face, hands and body, in the order named.

Angyal (6) shows that Katz' *surface touch* (Oberflächentastung) is bidimensional, while his *volumic touch quality* (raumfüllendes Tastquale) and *projected touch phenomenon* (raumhafte Tastphänomene) are tridimensional. In volumic touch quality, there is added to the surface touch phenomenon a sensory component of loose, incompact or spongy character. The projected touch phenomenon is described as having two surfaces, one of which rests on the skin and the other parallel to it but separated from it by an intervening loose, spongy mass. A dynamic component is added in cases of vibration or movement. A shift in the attitude of the subject may convert a volumic touch quality into a projected touch phenomenon, or vice versa. The attention is directed to the surface which is not in contact with the skin in the projected touch phenomenon.

An apparatus by means of which a bead fastened on the end of a small rod is moved over the skin at varying rates and intensities is described by von Frey, Fischer and Grundig (30). Movement is found to be more readily perceived at the higher velocities and intensities of the apparatus. At reduced rates combined with low intensity, the moving stimulus is ineffective or gives rise to illusions. The threshold of movement is not essentially lower than that determined by separate up and down movements of the stimulus. Allen and Macdonald (5) attempt to determine the critical frequency of touch stimulation. The stimulations of pressure, pain and temperature are periodically interrupted by a rotating disc. The results tend to indicate that all sense organs obey the same fundamental laws. Cold, and especially warm, were not very successfully aroused by this method of stimulation. Pressure fails to respond at high frequencies and also at an intermediate zone of frequencies. The authors believe that superficial pressure responds at one zone of frequency, and deep touch or pressure at the other. Pain responds only at low frequency.

Piéron (78) discusses the general status of our knowledge as regards the conditions of stimulation and dimensions of experience in the field of touch. He concludes that we know much less in regard to touch than in regard to vision, and that, as a result of the complexity of the problems and the lack of adequate facts, general laws of touch cannot yet be deduced.

Pain. Different qualities of pain are distinguished in several investigations. Schriever (88) excites pain by a cold stimulus and

finds that the threshold of clear pain is about the same in all parts of the skin, varying only and inversely with the thickness of the corium. The threshold of dull pain, however, varies in different parts; wherever nerve branches rest near the surface of the skin, the threshold is low, and wherever they are deeper seated the threshold is correspondingly higher. The two varieties of pain also show differences in regard to distribution in the mouth cavity. Sensitivity to clear pain is greatest on the tongue and gums, and least on the walls of the cheek and lips; that to dull pain, although greater in general than on the skin, shows only slight differences in the different parts of the mouth. Schriever (89) states that the nerves which mediate dull pain are more sensitive to cold and pressure but less sensitive to warm than those nerves with which clear pain is correlated. The latent time and after-sensation period of dull pain are as a rule longer than those of clear pain. There are no significant differences, however, in regard to adaptation, fatigue, and recovery from injury. Using electrically heated platinum wires, Schriever (87) determines the differential threshold of pain at different intensities. His discovery that the relative difference limen is about one-sixth or one-seventh at all intensities indicates that Weber's Law holds in intensive stimulation of pain.

Bazett and McGlone (8) puncture the skin, arteries, veins, deep fascia, periosteum, etc., with needles, and arrive at the conclusion that pain on the skin differs from that excited in the walls of the arteries and deeper tissues. The latter type of pain is characterized by a dull ache, is less acute, but also less bearable than superficial pain. Deep pain is also often accompanied by such reflexes as nausea, etc., which are under the control of the autonomic nervous system.

Kiesow (61) comes to the defense of von Frey's theory of pain as against that of Goldscheider, who asserts that tactile nerves mediate pain. He indicates a number of distinguishing psychological characteristics of pain and touch, and asserts that, in view of the manifold differences, separate sense organs for the two qualities is a necessary assumption.

Woolf (95) regards pain as the result of excessive stimulation of any sense organ. He launches invectives against the view of MacKenzie and others who think that, owing to the fact that in surgery the exposed organs are insensitive to pain, it is not a visceral experience at all, but is aroused as a result of transmission of impulses from the viscera through the nervous system to the somatic organs

in which the experience has its origin. Woolf points out that pain can actually be felt internally, and that the supporting evidence for MacKenzie's view consists of exceptional cases. Lapinsky (63) advances the view that referred pain is an instance of irradiation through the vascular centers of the thoracic cord.

Quantitative theory of feeling. Titchener's touch pyramid is criticized by Nafe (72) on the ground that certain dimensions represent qualitative differences and others quantitative or intensive gradations of touch, and that those experiences are in no real sense simple and unanalyzable. He points out that all cutaneous experiences are patterns of brightness of varying degrees of complexity, that they bear resemblance to timbre in tonal complexes, and that affective and emotional experiences are complexes of felt (sensory) experiences. Owing to the psychological kinship of cutaneous, kinesthetic, organic, affective and emotional experiences, Nafe suggests that the term "sense of feeling" is the most appropriate caption under which to classify them. In another place, Nafe (73) denounces the prevailing tendency to apply the doctrine of specific nerve energies in the field of touch on the ground that no satisfactory positive evidence in support of that view is available. Against this traditional view, which assumes separate sense organs for the several qualities of touch, he submits a quantitative theory (74) which dispenses completely with the concept of quality. The different qualities are accounted for on this theory in terms of quantitative variations of intensity, duration, density and extensity. Pain is always aroused at high, contact and tickle at low intensities of stimulation—per unit area—of the skin. The temperature qualities are less satisfactorily woven into the theory, but owing to the fact that the two thermal qualities are not coexistent and are seldom, if ever, aroused except along with pressure, it is suggested tentatively that they represent opposite and special modes of stimulation of the same pressure-pain nerves.

Touch localization. Cole (12) is concerned with the problem of errors in touch localization. When he uses Weber's second method, he finds constant errors in certain regions but not in others. The glass plate method, in which the localizing movement is terminated by a glass plate which rests above the skin, yields constant errors at all regions. Progressive stimulation, whereby the second stimulus is applied at the point at which the first one was localized, and the third in turn where the second was designated, gives results which exaggerate the tendencies to error characteristic of localizations by

the ordinary method of stimulation. As regards the matter of fatigue of pressure spots, Cole shows that a king-spot may be stimulated ten times in succession without resulting increase in the size of the error of localization. Von Skramlik (90) points out that his discovery that localization of pain is less accurate than that of touch contradicts flatly results published by Kiesow and by Ponzo. He believes that this discord in experimental results is not to be attributed, as Kiesow thinks, to a difference in the method by which the subject localizes the stimulated spot, but to the fact that Kiesow and Ponzo aroused complexes of touch in which there were both pain and pressure components, whereas he secured pure pain sensations. Mayer (70) finds that touch complexes of pressure and pain are more accurately localized than either pure pressure or pure pain; and that the error of localization is greater for pure pain than for pure pressure.

Two-point discrimination. Rein and Strughold (81) find that the threshold of duality for cold varies in different parts of the body. It is smaller than that of warmth in the same area, but is, on the contrary, always larger than that of pressure. Malamud (69) tests the two-point sensitivity to pain and to pressure, determining (1) the point at which the impression first begins to change into a distance, (2) the point at which the impression appears as a distance bounded by two points, (3) that at which the distance corresponds more or less accurately to the actual separation, and (4) that just above which two points are consistently felt. The pain values are lower than those of pressure in the first three, but in the last case the threshold of pressure is lower than that of pain. Friedman (31) finds that the spatial threshold of duality decreases up to the age of twelve, after which it increases. He believes the increment after twelve depends on intellectual rather than on anatomico-physiological factors. Richardson (83) discusses results of experiments on double touch and points out that best representation of the thresholds in cases where sensation is confused by vagueness and uncertainty is in the form of a regression curve giving mean sensation as a function of stimulus.

Tactual perception of form. The ability to apprehend form by touch has been investigated by several persons. Working on the volar surface of the forearm, Zigler and Northup (96) find that forms of tactually presented figures are not definitely perceived in more than about 50 per cent of the presentations; that triangles are more easily apprehended than squares, hexagons and diamonds; and

that the main dimension of a figure must be 12 to 15 mm. for the form to be perceived. Zigler and Barrett (97) find that outline figures are better apprehended than solid, and the latter in turn than point figures. The tip of the finger is a better receptor of form than the palm, which in turn is better than the forearm. The perception of form is not immediate, at least in a great many instances, but develops gradually through well delineated stages. Bose and Kanji (10) and DeGowin and Dimmick (20) emphasize the rôle of both visual and tactual imagery in the process of perceiving tactually presented forms. Révész (82) points out that visual and tactual processes are intimately fused in the perception of form; and that the parts of a whole are not spontaneously organized but are arranged into a form by a relating activity. Rosenbloom (84) also finds that closed figures are more clearly apprehended than open figures, and shows that a gap in a figure equal to the two-point limen in that part of the skin is not perceived as a separation or gap.

Post-operative sensitivity of the skin. Sir E. Sharpey Schafer (85) had the ulnar branch of the internal cutaneous nerve transected in the left, and the same nerve crushed with the forceps in the right arm. Owing to accessory nerve supply to the same regions, complete anesthesia failed to follow in either case. However, a condition of hypersensitivity, especially to pain, prevailed. Recovery of sensitivity was complete in four months on the side in which the nerve had been crushed, but in more than nine months the other side had not completely regained its normal sensitivity. Schafer (86) selects the little finger as the region in which complete denervation is possible. The nerve is crushed on one side and divided on the other. Total insensitivity results in both little fingers. Recovery is complete in three or four months on the side in which the nerve was crushed, but normal sensitivity has not taken place in more than fifteen months on the other side. It is suggested that in the former case the nerve sheath was not destroyed so as to prevent the ends from easily reuniting, while in the case of nerve division the reunion takes place, if at all, under less favorable conditions. Sensitivity to pain makes complete recovery first, and that to touch last. Schafer alleges that protopathic sensibility is an unnecessary postulation. Fifteen years after the operation, Duliére (21) tests the sensitivity of a patch of skin which was transferred from the arm to cover an ulcerated place on the cheek. The same type of sensitivity is found to exist on the graft as on the corresponding region of the other cheek, but the latter shows better chronaxie. However, the two

biceps, from one of which the piece of graft had been taken, exhibit the same chronaxie. Eidelberg (22) reports that in several cases of syringomyelia, sensitivity to either wetness or dryness is reduced without affecting the other quality, and that this altered sensitivity exists only in certain parts of the body. Hoff and Schilder (52) report that victims of *tabes dorsalis* or of lesion of the cerebellum, and to a lesser extent normal subjects sometimes, experience an underpropping of the fingers when the hands are extended in such a way that a small space is left between the fingers or when the arms are extended and the hands are closed together. It is suggested that this is an important clinical symptom of organic illness, owing to the fact that it is accentuated in cases suffering from lesions in the cerebellum.

Nature of discharge of sensory impulses. Several investigators present graphic records of the action currents which are released when the skin or a muscle is stimulated in various ways. While one investigator finds that the records of different modes of stimulation are essentially of the same type, others offer results which point to at least two varieties of discharge of sensory impulses.

Adrian (1) used a capillary electrometer to record the changes of electrical potential in the discharge of sensory impulses, in the nerve-muscle preparation when excited by tension, and in the cat's toe-pad when stimulated by pressure, contact, movement of hairs and prick of a needle. He finds that the action currents to pain and to pressure stimulation are of the same type. In all cases they consist of a series of impulses which obey the "all or nothing" law, *i.e.*, they do not increase in extent with increase of intensity of stimulation. Variation of intensity produces changes in the frequency of impulses only. His technique is described elsewhere also (2, 3), where he again asserts that all sense organs in the skin respond by the same type of sensory impulse discharge. No qualitative differences, but quantitative differences only, such as variation in refractory period or rate of adaptation, are found. Fulton and PiSüner (32) show that irregular action currents may be detected in the galvanometric records of an extensor muscle, such as the knee jerk or stretch reflex, if the muscle is placed under slight initial passive stretch before the reactions are elicited. In explanation of this finding, they determine that certain currents are occasioned by end-organs arranged in parallel with the tension yielding elements of the muscle (muscle spindles) while others are produced by end-organs arranged in series (Golgi tendon organs). McGouch, Forbes, and Rice (67) secure synchronous galvanometric records of the

tension of the *tibialis anticus* muscle and of movement of the foot. While their results point in the same direction as those of Fulton and PiSüner, that action currents arise from impulses released by more than one kind of receptor, they fail to find sufficient evidence to assign the components of the record to specific receptors.

Gasser and Erlanger (33) suggest a new method for locating the fibers of the different cutaneous senses in a nerve trunk. They induce nerve block by (1) cocaine and (2) pressure, and find that the nerves are differently affected by the two types of block. The order of disappearance when nerve block is established by cocaine is pain, cold, warm, contact. While the results are somewhat less conclusive when nerve block is induced by pressure, the indications are that the order of disappearance is just the reverse of that for cocaine blocking.

Galvanic skin reflex. The nature of the galvanic skin reflex has been studied by a number of workers. Darrow (19) finds that this reflex varies in extent for sensory and ideational stimuli. There is a marked rise in the reflex response when a sensory stimulus is presented, but only a slight change occurs when ideational stimuli are used. The same authority (18) offers three modes of explanation of the fact that a feeling of warmth or flushing accompanies the galvanic skin reflex. There may be (1) a momentary acceleration of metabolism, or (2) increased thermal permeability of the tissues, or (3) acceleration of the capillary circulation. Gildemeister (42) finds that the psychogalvanic skin reflex is occasioned by differences in the amount and rate of polarization in different parts of the skin. In another place (43) he says the skin acts as a polarization cell in which electricity is stored up by shift in ion potential. Hahn and Lueg (48) determine that thermal stimuli release this reflex in cold blooded (frog) but not in warm blooded (cat) animals. Jeffress (55) finds that tension of the arms or clenching of the hands affects the psychogalvanic skin reflex. The parts under tension are positive to parts of the body not under tension. Electrical potential is assumed to be more alike in symmetrical than in unsymmetrical parts of the body. A new electrode, which can be used with liquid or paste substances and which eliminates accidental deflections of customarily used electrodes for the psychogalvanic skin reflex, is described by Lauer (65).

The sense of vibration. Katz (57) again advances arguments in support of the view that the sense of vibration is the genetic antecedent of both touch and hearing. He shows (58) that the senses of

touch and vibration are distinguished on the ground that the latter has a significant dynamic component which the former fails to exhibit. In normal subjects, sensations of vibration are aroused along with auditory sensations which tend to obscure the sensations of vibration. But in deaf subjects, such as Helen Keller and Eugene Sutermeister, who are able to enjoy and to discriminate between different kinds of music, the rôle of the vibration sense is clearly indicated. Katz and Noldt (59) point out that they had no intention, in connection with their work on the frequency threshold of vibration sensations, to give the impression, which von Frey got, that sensations of vibration represent a special kind of sensation of movement. Just perceptible vibrations give rise to experiences in which the perception of movement is not directly perceived. Katz and von Götzen (60) arrange an experiment which puts to a test the physician's method of diagnosis of the condition of deep tissues. Figures are chiseled out of, or embossed on, a lead plate, which is then covered with a cardboard through which the form is to be tactually apprehended. The forms can be determined through the joint medium of vibration and auditory sensations, or they can be determined through the sense of vibration alone. The sense of vibration is here designated a distance receptor.

Petzoldt (76) performs experiments having to do with localization, threshold values and masking of vibration sensations. Localization is accurate both in the hands and in the feet, unless the members are crossed, in which case confusions of various kinds occur. Threshold values are of the same magnitude in the hands and feet. A stronger vibration sensation masks completely a weaker one, but if the former is terminated while the weaker one continues there is an illusory perception of movement; the vibration sensation appears to move from the point of stronger to that of weaker stimulation. McKinley (68) shows how a tuning fork may be adapted to the purpose of determining thresholds of vibration. Knudson (62) develops a technique for determining the sensitivity of the skin to vibratory stimulation. It consists essentially of a vacuum tube oscillator which generates frequencies between the limits of 10 and 50,000 per sec. With this instrument he determines the lower limit of sensitivity to vibration at 12 to 15, and the upper limit at 1,600 per sec. Gault reports in one place (36) that the upper limit is above 2,000, and in another (37) places it at 2,700 vibrations per sec.

Several kinds of teletactor are described by Gault (36). By the aid of these, deaf and normal subjects can learn to identify words in

terms of variations of weight or intensity, roughness and total vibratory patterns of their tactual impressions. Crane (15) finds that vowels and diphthongs can be discriminated through the vibratory patterns of roughness received through a teletactor. He finds an inverse ratio between intensity of tactual stimulation and ease of discrimination. Gault and Crane (40) use a multiple unit teletactor with which to determine the order of difficulty in understanding the different vowel sounds. Gault (38) shows that, by the aid of the teletactor, a deaf lip-reading subject can understand about twice as many words as he can by using lip-reading alone. In another study by the same author (39), homophenous words were presented to deaf subjects who depended in one case upon lip-reading and in another upon vibration patterns produced by these words as transmitted to the teletactor. Barring a few exceptions, the sense of vibration proves to be the superior avenue through which to present these words for understanding.

Kinesthesia. The majority of the studies which fall under this topic are concerned primarily with problems of learning or judgment of space. They indicate the influence which varying degrees of activity or kinesthetic tension have upon rate of learning or upon accuracy of judgment of size and distance. Two or three studies only have a direct bearing upon the problem of the nature of kinesthetic experiences, or describe apparatus by the use of which investigation of such a problem may be made.

Von Frey, Grundig and Strughold (29) repeat Cohen's experiment in which after anesthetizing the skin by the injection of novocaine the last named found the threshold of deep pressure to be twice as high as previous to the use of the anesthetic. Von Frey and his associates inject novocaine subcutaneously and intramuscularly, and find that the threshold of deep pressure is uninfluenced in both cases. An apparatus by which muscular contractions in the living subject can be recorded is described by Pritchard (79). A light aluminum frame is fitted to the patella horse-shoe and connected with the end of a spring balance from which readings of the extent of shortening of the muscles in knee jerk responses can be taken. Franz and Watson (26) have designed an apparatus by which force of movement can be measured. The subject pulls against a spring, from the scale of which readings are taken. The units of the scale may readily be changed for different experimental requirements.

In connection with learning experiments, the rôle of kinesthesia has been studied both in the human and in lower animals. Bar-

low (7) finds that grade school children and college students memorize lists of words more rapidly when permitted to repeat the words to themselves than when explicit laryngeal reactions are suppressed by the act of holding a pencil firmly between the teeth during learning. The order of efficiency of the different methods of attack in memorizing the Miles elevated finger maze is found by Husband (54) to be: (1) verbal or counting, (2) visual, (3) combination of verbal, visual and motor, and (4) motor. Cox (14) finds that individuals who score higher in the Thorndike intelligence test tend to rely more on kinesthetic than on visual or mixed guidance cues in their performances on the Miles two-story maze. In performances on the same maze when the lower pattern is disoriented 90 to 180 degrees, the same writer (13) finds that reliance upon visual cues to be superior to that upon tactual or kinesthetic cues. Lashley and Ball (64) trained rats to run a maze, whereupon sensory function was altered by complete division of the dorsal or lateral funiculi. The animals were still able to run the maze in spite of serious sensory disturbance, but control experiments seem to indicate that these post-operative performances are mediated by some intraneural mechanism rather than by any directive kinesthetic or proprioceptive sensory cues. Johnson (56) studies individual differences in regard to the amount of pressure characteristically exerted in certain motor tests, such as tapping, and speed and accuracy of movement. Every subject tends to conform to one of the following types: (1) slight pressure throughout the performance, (2) initial rise in pressure with gradual decrease, (3) slight initial pressure with gradual increase, and (4) irregular changes in which high pressure tends to predominate. By clipping the vibrissae of rats, Fields (23) finds that tactual and kinesthetic factors have no determining influence upon the discrimination of form by the rat.

Other experiments throw light upon the way in which kinesthesia influences the phenomenology of space and time. Weber (93) determines that a distance reproduced on the Michotte kinesthesiometer under load appears to be longer, proportional to the amount of load, than one of the same physical extent without load. He also finds that the same interval of time perceived under strain appears longer, proportional to the amount of activity, than when it is passively perceived. Weber and Dallenbach (94) had their subjects trace areas, angles and arcs with differently weighted types of stylus. They find that, when traversed under load, the area appears to be enlarged,

the angle to be more acute and the arc to be more sharply curved than when traversed without load.

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THE PERCEPTION OF VISIBLE MOVEMENT

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During the last two years the literature on the perception of visible movement has been concerned chiefly with (1) intensive studies of apparent and real movement in order to derive explanatory principles for the perception of movement, (2) further observations on various visual illusions and, (3) theoretical papers on the perception of movement. There has been a continued tendency to explain the perception of visible movement on a phenomenological basis.

I. EXPERIMENTAL

De Silva (3) finds "that visual movement produced by successively appearing parallel lines, depends upon a complex arrangement of stimulus determinants to maintain it as a good illusion." The parallel lines were presented kinematographically. Experiments were carried out concerning the perception of movement as regards the size variable and such other factors as influence of varying intensity of light, addition and subtraction of members (stimuli), influence of varying pause, influence of varying exposure times, focussed vs. unfocussed stimulus, influence of varying distance between the lines, crosswise vs. up-down movement, influence of coloring stimulus, foveal vs. peripheral movement, and simultaneous movement in opposite directions. The following precautions are suggested for a study of the perception of movement: "Unless conditions are correctly arranged the phenomenon ceases to be perceived as movement and becomes inferred. Judgments upon optimal apparent movement made with a gross stimulus attitude are likely to be unreliable because of the influence of secondary attributes such as swiftness, jerkiness, greyness, etc. O's may wittingly or unwittingly adopt one or another aspect of the perceptual pattern as their primary criterion of judgment. Therefore, because observable secondary attributes vary as the stimulus varies, and because an O's criterion for judging optimal movement may vary, it follows that we should rely altogether on gross judgments upon the stimulus which produces ap-

parent movement." It is found that the time element is the most fundamental determinant of movement. The angular velocity, as measured from a subject's eye, and the duration of the sensation of movement are also greatly significant. The illusion of movement further depends upon such additional factors as the length of line, size of stimulus, brightness contrast between stimulus and background, color of stimulus, and region of the retina stimulated. In another investigation de Silva (4) studied the visual perception of movement by the use of a special apparatus which permitted the exposure against a black background, of two bright lines. The time interval between the exposures of the lines as well as their distance apart could be varied. Occasionally a line was introduced when the first line disappeared, moving towards and disappearing when the second line appeared, thus giving the sensation of an actual moving object. On other occasions the two lines alone were shown successively, with no actual movement of an object in the space between them. Experiments were conducted in order to determine the effects of physical differences, *i.e.*, variations in the distance between the lines; variations in the speed of movement and the length of the pause between the exposures of the two lines; variations in the exposure time and light intensity, and (2) to study some of the effects of central determinants, such as physical clearness, expectancy, sets, etc., on the perception of movement. Movement is characterized by its two fundamental attributes, namely, "movingness" and the "vehicle-of-movement." "Movingness" is closely related in experience with the attributes of extensity and protensity, and is comparatively independent of the attributes of quality and intensity; on the physical side it is most directly comparable to physical velocity. The "vehicle-of-movement" varies greatly in clearness. The relative clearness or obscurity of the "vehicle-of-movement" marks off the chief distinction between slow and fast real movement, and between real movement and apparent movement. The best conditions for the appearance of the illusion of apparent movement are short distances between objects, short pauses between exposures, and to a lesser extent, long exposure times and increase in the light intensity, while with real movement at high velocities the perception of "movingness" may disappear. The effects of the central determinants are emphasized as of marked importance. Under favorable conditions a single stationary line may be seen to move, a moving object may be seen as stationary, or movement may appear to be in the direction opposite to the actual direction. Introspection is found to be inade-

quate to give a complete account of the perception of movement, since so many significant details are never clearly represented in consciousness. Head's theory of schematism together with the help of three further concepts, *viz.*, attention, clearing-up, and clearness-status, serve as principles of explanation. Dimmick and Sanders (5) made an intensive study of the influence on the phenomenon of visible movement of two factors, *viz.*, the length of the temporal interval and the intensity of the stimulus; 0σ , 30σ , 60σ , and 90σ served as time intervals, whereas "brilliant white," "good white," and a "dull white" denoted the three intensities used. The subjects were instructed to give an accurate description of the visual perception. Results show that the shorter time intervals give simultaneity, the longer ones succession, and the intermediate ones movement. However, when the instructions were changed to read, "You will be shown a stimulus which may or may not arouse a perception of movement, state whether you perceive movement," the percentage of perceptions for all the subjects was more than doubled. The former type of movement is called the "apprehension of movement" and the latter the "inference of movement." "Visible movement" is another type of movement whose basic sensory correlate "is an unsteady or flickering bulky-film of gray which in the case of the stroboscopic stimulus, consists primarily of projected 'central gray' modified in a particular case by the qualities of the stimuli and of the background and by perceptual images of the order of 'memory color' or 'tied images'." The frequency of optimal movement varies with the intensity of the stimulus, the percentage of cases of optimal movement at 30σ and 60σ time interval being increased by an increase in the intensity of the lights, but the result is not univocal. The results are in general agreement with those of de Silva (3, 4) and Higginson (18). In a study on reciprocal inhibition and reinforcement in the visual and vestibular systems, Travis (31) observes that under certain conditions the visual and vestibular systems seem to be active even though there be no immediate objective stimulus of movement. The apparatus used in this study made possible a simultaneous or separate movement of body and a fixated object. It lent itself to an analysis of the following conditions:

- (a) Visual object and platform moving in same direction
- (b) Visual object and platform moving in opposite direction
- (c) Visual object oscillating, platform stationary
- (d) Platform moving, visual object stationary

The observed hallucinations of movement of the body and visual

object is believed to be conditioned by a central factor possibly in conjunction with one or more peripheral factors. "The central factor appears to be very closely connected with both positive and negative responses and in certain instances seemed to re-enforce and in others to inhibit the peripheral excitatory processes." Guilford and Helson (15) obtained photographic records of eye-movements during the perception of the phi-phenomenon, which they interpret as showing that eye-movements do not play any essential rôle in the perception of hallucinatory movement. These findings are opposed to Guilford's earlier findings and those of Fisher (11), Stern (30), and Higginson (19), and others who accept the eye-movement theory as an adequate explanation for stroboscopic movement. It is interesting to note that Guilford's and Helson's and Higginson's subjects agreed in their introspective reports. Both sets of subjects report eye-movements and strain and kinesthesia during the perception of stroboscopic movement. Guilford, however, believes that "the only court of appeal is the objective record of what the eyes were actually doing at the time the movement was seen, unless one resorts to the theory that the phi-phenomenon is due not to overt eye-movement but to strains and incipient movements." It is further suggested that any general theory of apparent movement must take account of all the facts, not only of visual, but also of auditory and tactual movements from stationary stimuli. Rutten (29) obtained some interesting phenomena of visual movement in a study of the Müller-Lyer illusion. The elements of Müller-Lyer's images were successively and tachistoscopically cast on a white screen in a perfectly dark room. At the exposure of the figure and its parts, regular changes and apparent movements took place in the background. These movements are separated into (1) "phenomena which are the consequence of the tachistoscopic exposure of an object, and (2) form-phenomena, which must be considered as a result of the tendency to the formation of a new whole from collective data. . . . The movement which emphasizes the difference in length between the isolated line and the line of the figure is a consequence of the sudden apparition of a complex of which the spatial relations are overrated." Dunker (7) distinguishes between "induced" and "experienced" movement. Induced movements are those in which the objective stimulus remains stationary and the experienced movement is due to a shift of the surrounding objects or of the observer. When directing a stationary point of light on a large card and then moving the card to and fro, it seems that the point of light also moves.

Similarly, when two rows of several parallel lines are placed one above the other, and one of the rows is moved the other seems to move. In all cases, "phenomenal movement is a displacement in a natural system of reference." Guilford (14) gathered data from three observers to test the eye-movement versus the streaming theory for the explanation of autokinetic sensations. The moving spot was observed to leave a trail of gray or of a color complementary to that of the spot. Observations show that there is movement of a film over the whole field in an opposite direction to that of the spot, and that this film can be seen in various tints drifting across the face of the spot. The flowing streams are not those of Edridge-Green or of Ferree, they are streams which involve the entire retina at once. The spot moves toward and the film away from eye pressures. A change in eye-pressure produces a corresponding change in the direction of the spot. Streaming is not the cause of the eye pressures in question, because artificial streaming set up by a kymograph drum induces pressures in the same direction rather than in the opposite direction. Initial movement of the spot is about as often outward as inward. Evidence is found that differential fatigue and recovery of extrinsic eye muscles do not determine the direction of movement of the spot. Zietz and Werner (33) find that rhythmically or arrhythmically presented auditory stimuli influence the perception of optical movement. Various figures containing arrows, irregular lines, and circles, were presented stroboscopically, accompanied by rhythmical or arrhythmical tapping. It was found that auditory stimulation elicits optical movement or influences the form of the movement or of the moving object. Auditory stimuli exert a modificatory influence upon the perception of optical movement to the extent of changing the course of visual movement. Movement is believed to be the dynamic function of the entire organism. In a study on the visual perception of movement Bonaventura (1) confirms Exner's results which lead to the conclusion that the visual perception of movement does not presuppose the perception of a succession, such as luminous points. In Exner's sense this does not necessarily imply the existence of a specific "sensation of movement." There is no justification for calling the apparent change of object a "sensation of change." Ehrenstein (9) formulates the following laws of the perception of movement on the basis of material previously presented by him and others:

1. Law of activity of contiguous retinal areas. (*Umfeldwirkung*)
2. Law of *Einstellungswirkung*.

3. Law of the spatially-modifying force (*raummodifizierenden Kraft*) of primary and secondary movement.

Perception of movement belongs to the general class of experiences of relationship. Configurations arise out of a background of manifold visual forms. If one of these forms differs from the uniform rest in color, form, size, familiarity, in having long and thin points, or in being singled out by the observer, then it becomes the figure in relation to which the forms constitute the background. Musatti (25) presents data which do not bear out the conclusions reached by W. Ehrenstein in his studies on apparent movement. Ewert (8) inverted the visual field of view 180 degrees by means of specially constructed prismatic lenses. It was found that under these conditions eye- and head-movements resulted in a general rocking and swimming effect of the whole visual field. This movement was very apparent for several days during continuous inversion, then became less noticeable, but it appeared again after removing the lenses. Movement of visual objects was reversed from normal movement and much accelerated. The illusion of reverse movement was so strong that it could not be verbally inhibited even when the eyes were temporarily closed. Movement under these conditions is ascribed to the antagonism between old (normal) and new (necessitated by inversion) eye movements and the resulting antagonism between eye and head or body movement. Normally, compensatory eye reflexes keep the eyes fixated on an object during head movement. Under inverted conditions, however, due to the antagonism between eye and head reflexes, the new (necessitated by inversion) compensatory eye movements cannot keep pace with the head movements. It was also found that this new visual movement greatly influenced normal auditory movement. The following is a report from one of the subjects: "A car was heard to come from the right, moving toward the left. As soon as it entered the field of view the sound jumped over to the opposite side. It was now both seen and heard to travel toward the right. However, as soon as it passed out of the field of view the sound seemed to jump back and continued its course toward the left." Rexroad (28) suggests a theory that visual images and their movement are determined largely by the tensions in the recti muscles. It was found that if a subject fixated a point at one side of a large light spot and the spot was made to disappear, the after-image appeared to float away from the point of fixation. By testing fixation after some seconds it was found that eye-movement had occurred in the direction of the apparent movement of

the image. If eye movements took place in the opposite direction before the image appeared, its appearance would be delayed, and if they were set up after the image appeared it would be abolished during the movement. Guilford (17) describes a phenomenon of the momentary resting appearance of a revolving wheel in linear motion. It was observed that the wheels of a passing motor car were spasmodic in their movement, proceeding by alternate stops and starts. By fixating the hubs of the moving wheel, the spokes were seen to flash out in complete detail for a moment, the whole wheel appearing stationary. The eye movement theory is proposed as an explanation and then is further tested in the laboratory. A wheel was constructed from a black cardboard disc 25 cm. in diameter, with a white center 6 cm. across, and 16 white spokes each 1.2 cm. in width. The wheel was driven by a motor and its speed controlled by a rheostat. The speed most favorable for the production of the phenomenon was found to lie in the region of speeds at which flicker ceases and color mixture begins. The following report of one of the subjects exemplifies the nature of the observed phenomenon: "I got the spokes very definitely. Occasionally I got things like lightning flashes, light gray bars against a dark background. When I winked I saw all the spokes very clearly all the way around; it seemed to stop dead still." It was found that most of the flashes were accompanied by eye movements. But a considerable percentage of them were not. Since the stationary flashes seem to come rhythmically, it is proposed that time may phenomenally flow at a non-uniform rate, proceeding by starts and stops. And this, it is believed, may be in part responsible for the stationary phases in the turning wheels. It is further suggested that the eye movements themselves may produce the distortion of time. A similar phenomenon is described by Gradle (13), who reports that the propeller of an airplane whose tachometer reading swung between 1,650 and 1,675 revolutions per minute furnished the stimulus. The phenomenon was not obtained with direct vision. But as the visual axis was turned laterally (either right or left), a blur in the form of the arc of the propeller became visible. Upon increasing the angle, the general outline of the propeller blades became definitely visible. Between 40° and 50° , the blades were seen as individual, and for about 10° to either side of these limits, the blades were seen as a blur. No explanation is offered.

II. VISUAL ILLUSIONS

Gaehr (12) and Packard (27) describe the "spoke illusion." Gaehr offers the theory that all our time experience is "quantitized." Ferree (10) offers the eye-movement theory as an explanation of the phenomenon. "When successive impressions are given to any portion of the retina at a rate which produces a fused or continuous sensation, a quick lateral movement of the eyes causes momentarily a resolution of the fusion into component sensations. . . . The movement of the eye momentarily interrupts the succession of the impressions on any given portion of the retina, therefore the combination of impressions is prevented and the components are sensed as separate." It is suggested that the illusion is easier to get in indirect vision because the limen of fusion is higher there than in direct vision. The illusion is enhanced by distant accommodation because this means a larger pupil, a more intense stimulus, and hence again, a higher limen of fusion. Guilford (16) describes the following three forms of illusory movement in a rotating barber pole: (1) The subject may see the tortional movement as it actually occurs. (2) The subject may see movement in the vertical direction only, horizontal movement is missing. "The total perception is of a non-rotating pole above and below the glass case." (3) "The observer may see a combination of horizontal and vertical movement, with one of the two components prevailing to a greater or less degree." Dufour (6) found that when fixating a star with an opera glass (eight-fold magnifying power) it was seen as unsteady and as moving slightly in all directions, whereas the stars falling on the periphery of the eye appeared immobile. The movement in the direct field of view was found to be due to the involuntary movements imparted to the glass by the hand. Lack of movement in the periphery is ascribed to the differences of visual acuity in the macular and peripheral regions. Miles (22, 23, 24) gives a clear description of a number of commonly observed illusions, such as those of the windmill, the motion picture, the waterfall, the picket-fence, the firelight (phi-phenomenon) and several associated with the movements of railroad trains. The windmill illusion is explained as due to a "reversible perspective" of figure and ground. Since the observer is accustomed to seeing a wheel continue to rotate in the same direction, the seen change in perspective of foreground and background causes the illusion of a reversible motion. The waterfall illusion is believed to depend on the objective condition of movement

of relatively large areas of the visual field. The visual experience rests on the tendency for the visual field to operate as a compensating system, balancing its parts one with reference to the other. Kampmeier (21) describes some of the conspicuous phases of the phenomenon concerning retinal pressure images and their Brownian-like movement which was first described in detail by Purkinje in 1819. Pressure upon the closed eyes is observed to produce a fantastic play of light and dark figures, the vividness of which depends upon a number of factors, such as the state of rest or fatigue of visual elements. The dominating impression is an involved checkerboard design consisting of thousands and thousands of facets. These are arranged with remarkable regularity and symmetry and the sequence in which these designs follow one another is unusually constant. The individual units oscillate or quiver with the rapidity and degree of excursion of Brownian motion. The geometrical harmony of the images suggests that they are related to the retinal cells.

III. THEORETICAL

In considering various theories of visual perception of movement, Triepel (32) raises the question whether the physiological processes of the stimulated end-organ give rise immediately to the perception of movement or whether an additional central process is necessary to achieve this end. The possibility that the structure of the retina itself may contain features that further immediate perceptual movement, is then considered. It is pointed out that several visual cells converge upon one bi-polar cell and several bi-polar cells upon one ganglion cell. The discontinuity of the excitation of the visual cells caused by the moving object is thus overcome by their flowing together in the inner layers. Since the retina is an extension of the cortex it may be assumed, following Ebner's suggestion, that the image of movement is directly created in the retina and then rises into a conscious perception by associative connections in the occipital lobe. Cords (2) criticizes Leiri's theory of the after-image of movement explained in terms of after-nystagmus on the following grounds: The after-image is a purely local process; it is confined to the region in which movement has been seen. The after-image varies with retinal locality. Its speed decreases when contours and contrasts are eliminated, and when the duration of observation is shortened; optokinetic nystagmus is not affected by these factors. Cords then advances the theory that optokinetic after-nystagmus is a consequence of the after-image of movement. In a critical supplement

to the theory of the immobility of objects in case of voluntary eye-movements, Hillebrand (20) defends her previously advanced theory of "absolute localization" (the fact that the apparent position of an object remains constant although its corresponding retinal area has changed) against the objections of E. Kaila, M. Wertheimer, and F. B. Hofman. Hillebrand holds that the process underlying absolute localization furnishes the key to the explanation of stroboscopic movement. Apparent movement resulting from the exposure of two stationary stimuli must be regarded as an "elementary" phenomenon with respect to stroboscopic movement. In explaining absolute localization use is made of the fact that apparent movement is not observed in the case of voluntary eye-movements. The displacement of the visual field does not affect the "absolute" position of objects; only the relative position of changes. In a theoretical discussion concerning the problem of geometrico-optical illusions Oesterreich(26) questions the justification for speaking of optical "illusions" from a phenomenological standpoint. We are not justified in identifying "physical nature conceived of in a Euclidean sense and the immediately given, sensory contents of consciousness." Lines are straight or curved according to whether they are straight or curved perceptually.

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THE PSYCHOLOGICAL BULLETIN

A REVIEW OF RACE PSYCHOLOGY

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Five years have elapsed since the last review of race psychology appeared in the BULLETIN.¹ That review covered a period of eight years. Consequently the literature of that period would be expected to be more extensive than that of the five years just past. But if one may determine the matter by length of bibliography,² the past five years have been the period of greater activity. This is true not only of race psychology as a popular theme, but as a field of scientific study as well.

The last review sought to show trends in thought and in method of investigation. That is likewise our intention in the present review. After calling attention (1) to the trends in theory and experiment, we shall give some account (2) of the popular, theoretical and scientific anthropological literature dealing with race psychology; and then follow these with (3) a detailed account of that literature which deals with experimental and statistical treatment of the problem of racial differences in mental traits, since 1924.

(1) *Trends*. Many of the current trends have been noted in previous reviews, but some are only just now making their appearance. Some of the old movements have gathered momentum and some have languished. Some of the new trends are rather weak, while some are showing great vigor. Most of the trends are the

¹ A Review of Race Psychology, THOMAS R. GARTH, *Psychol. Bull.*, 1925, 6, 343-364.

² The writer begs to acknowledge the coöperation of his assistant, Miss Winifred Rose, in arranging the bibliography for this review.

natural outcome of various phases of activity and a few are rather difficult to account for and come as a surprise. Briefly we list the trends below:

- (a) The interest in the general subject of race psychology.
- (b) Belief in the hypothesis of racial inequality.
- (c) The perennial interest in primitive man.
- (d) The tendency for psychologists and anthropologists to work together.
- (e) The study of racial esthetics.
- (f) The use of experimental results of the psychologists by popular and anthropological writers.
- (g) The study of race mixture and its effect on mentality.
- (h) Psychoanalysis and race.
- (i) Biographical approach to race psychology.
- (j) The study of racial attitudes, or race prejudice.
- (k) Skepticism as regards the truth of the hypothesis of racial differences in mental traits.
- (1) Various trends in the experimental attack.

(2) Popular, theoretical, and anthropological treatment. It is to be noted again, after five years of activity, that the experimental and statistical attack on the problem of racial differences in mental traits is the method by which racial psychology moves rapidly forward. This continues to be gratifying to the experimentalist. Nevertheless, the theoretical studies and popular presentation of the problem are active in rendering assistance of a most valuable sort. Such is indicated by their insistence on keeping the matter before the public, in penetrating new areas lightly esteemed at first by the experimentalist, in pondering carefully the findings of the experimentalist, in bringing new forces of endeavor into the investigation.

As for the anthropologist, who with the race psychologist has a common scientific interest, the task would be practically hopeless without his assistance. There is to be noted a somewhat more active interest on the part of these scientists indicated by a helpful participation in investigations, and a valuable scrutinizing of the results of findings.

An examination of the appended bibliography and reference will reveal that there are listed at least twenty-four books, some more or less popular, but some quite technical. Because of the limitations of space it is not possible to mention many excellent books and articles excepting by title in the Bibliography and Reference.

Starting then with the attempt to divide the writers of these books and articles into those believing, and those not believing in the Racial Mental Difference Hypothesis (34) we seem to find a trend toward skepticism with reference to this hypothesis. However, many are not much concerned with native differences, but are interested in differences as they exist as being due probably to nurture.

Koehne (85) brings to light surprising evidence that Disraeli was the forerunner and inspiration of Gobineau in his theories involving the hypothesis of the inequality of the races of men.

Lévy-Bruhl (92) maintains his former belief in essential differences between the primitive and the civilized mind, and he is insisting that among savage peoples the individual is essentially lost in the group.

The original position of that writer, belief in essential inequality, is definitely contested by Allier (2) who claims that the differences between primitives and civilized peoples are produced either by lack of proper stimulation, or by the retarding influence of magic which has prevented, in either case, the development of the primitives. The civilized and uncivilized, he thinks, must have sprung from the same ancestry. Consequently they must have basic identity. This writer on the other hand, agrees with Lévy-Bruhl that magic dominates the mind of primitive man (3), but disagrees with the latter in believing that this is necessarily a stage that cannot be overcome, and that it has prevented logical thinking as well as moral integration among those peoples. According to Allier the differences correspond to differences in advancement. He notes that civilized peoples, too, believe in magic, but only to a less degree than primitives.

Contrary to both Lévy-Bruhl and Allier, Radin (138) believes there has been an overemphasis on this phase of primitive thinking, for if we disregard ritual and art, he sees no evidence that mysticism and symbolism dominate the life of the primitive man. While it may appear that the primitive's thought is non-logical, a close examination proves this to be incorrect. Radin holds that the assumption that primitive mind is at a dead level of intelligence, that such minds are bound down by group thought, that there is no intellectual class, is a gross misstatement of the facts. To quote: "No qualified observer of the customs of primitive man has ever denied the existence of thinkers among them" (138, p. 385).

The book, *Psychology and Ethnology*, by Rivers (142), who until he was well established in psychology, found no particular interest in ethnology, is a most attractive one. He was peculiarly prepared

to write on the psychological phases of ethnology since he organized and carried out the "first systematic fieldwork in the experimental psychology of primitive people" (Introduction XI—G. E. Smith). Rivers takes issue with Lévy-Bruhl in thinking that the mind of primitive man is "prelogical" as distinguished from ours which would be called logical (p. 53). "I may say that in intellectual concentration, as well as in many other psychological processes, I have been able to detect no essential differences between Melanesian or Toda and those with whom I have been accustomed to mix in the life of our country."

Nutting (123) who traveled much among primitive peoples says he is convinced there are no racial inequalities, also that there are no differences in brain structure.

Boas' work on primitive art (10) affords the student interested in racial esthetics a storehouse of valuable information, and a superb organization and systematization of facts and principles. It would be impossible to give a review of this book of any adequate proportions here. The conclusion to be drawn is that the same esthetic principles apply to all art. Luquet (99) insists that the art of primitive peoples reflecting their mental life shows them to be on the level of children. However, the reviewer is sure many would disagree with this conclusion. Somewhat in keeping with this view is that of Marsumoto (105) who indicates that the progress of art among peoples follows after the progress in art of the individual. If this is considered after the fashion of a learning curve, he believes that the art of civilized peoples is arrested upon a dead level. Rattray (137) points out that, especially in the case of the Ashanti, art and all phases of primitive life are closely interwoven with their religious life and activity. Following this as an illustration we have Renaud's account (138) of prehistoric female figurines indicating that prehistoric man in Asia and Europe were devotees of a female benevolent divinity. Bunzel (15) in a study of primitive art particularly with reference to its creative phase believes the Pueblo potter follows a blind unconscious lead in his better artistic production.

Crawley (19) in "Studies of Savages and Sex" gives a treatment of sexual anthropology on the psychological side especially. Here he discusses savage forms of love, the kiss, chastity, and of sex life of savages generally. This phase is also well handled by Malinowski (103). G. B. Johnson (79) after a thoroughgoing study of "Negro Blues" which are so much in evidence among present day popular songs, in addition to research into Negro life, particularly

into Negro vulgar expressions, concludes that these "popular blues songs" have a double meaning. He calls attention to the fact that the educated Negro is opposed to the "Blues Songs" which is possibly an implicit recognition of their deep obscene undercurrent.

Mead (108) deals especially with the adolescent psychology of some fifty Samoan girls. She finds them rather free from conflicts in ideals because of the freedom of sexual life permitted by certain cultural factors.

The question of racial purity is one of considerable importance in the study of racial psychology. We cannot speak of a race if we know nothing of its purity, its composition. For this reason the anthropological study by Chu Li (94) on the racial composition of the Chinese is of peculiar interest. In this study the investigator and author lays out the characteristics of the more typical Chinese, the "We-group" as compared with the "You-group," a group of less typical and more varied characteristics. He finds the best distinguishing trait to be the shape of the head. It is objected by Terry (156) that the factor of race composition has not been recognized sufficiently in the study of the American Negro. The mulatto may be as much White as Negro, but society makes him a Negro. Fleure (32) points out, apropos of our disposition to classify individuals on a basis of skin color that the skin of man has adapted itself to the climate by either becoming thicker and depigmented as a protection against the cold, or of a looser texture and of a darkened color as a protection against heat. Weidenreich (167) raises the question as to the cause of various body-build types found in the United States. They may be due to racial differences rather than differences in constitution.

Herskovits (66 and 67) by making genealogical investigations found only 20 per cent of American Negroes to be of pure blood, the mixtures being with Whites and Indians. In so far as mating between Whites and Negroes is concerned, whether legitimate or not, it has decreased. But among Negroes and mulattoes there is a preference for mates of similar color. Herskovits investigated to find the influence of white blood as indicated by various somatological characteristics (skin color, lip thickness, nose width, etc.) and genealogical records, and found no significant correlations. However, his group was a select one.

Boas (9), while endeavoring directly to show the uses to which the science of anthropology may be put in assisting human progress and life, does not neglect to note the relation between psychology and:

anthropology. With Boas it seems almost a hopeless task ever to indicate any racial differences in mental traits because of differences in individuals' experiences within a race, and because of native differences among individuals which are greater than the group differences. It is intimated that there may be a basis for racial differences running with somatological differences, but this is not very clearly indicated.

While anthropologists are endeavoring to coöperate with the experimental psychologist, there are instances in which these workers discredit the results of mental tests. Boas (9) points out that since the tests measure functions only it is difficult to know when these measure native character, and Estabrooks (28) insists that intelligence testing has failed to satisfy the anthropologist. On the other hand it is pointed out by SenGupta (149) that psychological and psychophysical tests afford the soundest approach for work in the field of race psychology, though no single method should be used alone.

The oft repeated caution is again sounded by Wallis (164) that it is difficult to interpret results of mental tests on such primitives as the Zulus because of the standardization of these tests on Anglo-Saxons.

Roback (143), discussing the possible inferiority complex of the Jews, concludes that they possess such a complex. He thinks that little can be done about it, however, excepting to face the situation frankly.

McCartney (107) claims to have found various symbolisms in prehistoric Chinese art and suggests that when the Chinese libido has become "freed," there will be a decided awakening for these people. Travaglini (160) after an examination of one hundred men and women among the Javanese concludes that this race is in a "primitive, catathymic, animistic stage." In consequence numerous mental diseases have a development that is different.

One of the interesting movements in race psychology is racial autobiography. Two books as frankly intimate from an introspective standpoint as the famous Confessions of St. Augustine have made their appearance. One is the inner picture of the mental life of a Black Ulysses, particularly the moods popularly characterized as "blues," with a title, *Rainbow Round My Shoulder*. To Odum (124) we are indebted for this remarkable contribution. The other is a frank and intimate reminiscence of an American Indian edited by Radin. The title is *Crashing Thunder* (135). The editor finds evidence of wish-fulfillment, disposition to commune with spirits.

There is a remarkable account of how the hero ate peyote and his introspections consequent upon the deed.

Popular literature is taking hold of this interest, and so there have appeared some pseudoautobiographies. Some of these are quite remarkable, and one might wish them to be authentic because of the striking truths which they contain.³

Reuter (139) indicates a growing of race prejudice among Whites and Negroes. While his book is more or less historical and sociological, it indicates a growth of race consciousness among the Negroes. Race progress on the part of the Negro is being hindered by race prejudice.

Frobenius undertakes to trace certain Negro potentialities from early African cultures to ascertain whether these can endure when transplanted. De Noyelles (24) observes that as between the White and Negro unskilled laborer the White is the better worker especially when no definite specified task has been set.

In *What the Negro Thinks* by Moton (113), himself a Negro, we have a very intelligent presentation of the Negro's reaction to his American social, economic, and intellectual environment. It may well be regarded as a true picture of what the educated Negro thinks as well as an account of his aspirations.

One of the most interesting trends in both discussion and experimentation is that of inquiry into race prejudice and racial attitudes. The question is raised as to whether or not the prejudice is of the nature of an "instinct." Lasker (90) while granting that children may have a native aversion to people of a strange race concludes that it is of slight degree at best, and grows stronger with increasing years. The early state of affairs instead of being abated by adult contacts is made all the more ingrained. However, the influence is so subtle that he is not aware of how it came about. Park (126) considers prejudice an attitude which is the mark of resistance on the part of the social order to change, and this is due to something more or less "instinctive" which may be described as consciousness of difference giving rise to "a vague apprehension." Race prejudice, it is pointed out by Park, may at one time have been advantageous to a given race in leading to solidarity, but now it promises to be a dangerous "instrument."

That race prejudice does not have the criterion of universality to make it an instinct is shown by the statement of Hobhouse (70) that

³ Cf. *The Autobiography of an Ex-Colored Man*, by JAMES WELDON JOHNSON. Alfred A. Knopf.

TABLE I

DATE	INVESTIGATORS	RACE	No. CASES	TEST	RESULTS
1925	Garth and Whatley (39)	Negroes	1272	N.I.T.	Med. I.Q. of 75.
1925	Mercer (111)	Negroes	1006	Color Pref.	Sequence: B, O, V, R, Y, W, G.
1925	Sunne (154)	Negroes	292	Will Temp.	Racial profiles similar. Striking individual differences.
1925	Murchison and Burfield (114)	Whites	232	X-O	Negro criminals are superior to Negro non-crim.
		Negroes	370	Army Alpha	Whites excel in ingenuity. Whites more speedy.
1925	Peterson, J. et al. (129)	Whites	74	Rational learning	
		Negroes	48	Ment. Maze Disc. transfer	
1926	Bond (12)	Negro	175	Color Naming	No correlation between intelligence and emotions.
				Terman Gr.	
				Otis	
				Pressey X-O	
				Downey Will Temp.	
1926	Herskovitz, M. J. (66)	Mixed blood Negroes	115	Anthr. Meas.	No significant correlations.
				Thorndike Col. Ent. Exams.	
				Army Alpha	Women are lower than men.
1926	Murchison (115)	Negroes, women	41		
		men	184		
1927	Arlitt and Buckner (4)	3 yr. Negro	23		
		3 yr. White	24		
1927	Graham (57)	Negroes	3028	Color Pref.	Red and blue pref. to yellow and green.
				Stanford-Binet	
				Kohs Block	
				Lincoln	
				Holden Sq.	
				Healy Con.	
				Color Pref.	Diff. between Neg. and Whites. Uneven with age except in lower grades.
1927	Hurlock (75)	Whites	206		
		Negroes	194		Blue and pink most preferred. I.Q. makes little difference.
1927	Witty and Decker (169)	Whites	1725	Stanford	Educ. retardation of Neg. Young
		Negroes	220	Achievement	nearer White normals.
1928	Davis (23)	Negroes	222	Terman Group	Med. I.Q. 78. Influence of education evident.
1928	Davenport, C. B. (21)	Negroes of the West		Army Alpha	Whites excel Negroes.
		Indies		Goddard	
		500 adults		Binet	
1928	Koch, H. L., and Simmons, R. (84)	Whites	1211	Meyer Pant.	Rank in excellence: (1) White, (2) Mex., (3) Neg. City excel rural
		Mexicans	1492	N.I.T.	subjects.
		Negroes	613	Pint. Cun.	Problems of folk music can now be solved.
1928	Metfessel, M. (112)	Negro		Phonography in Folk Music	Whites superior to Neg. Mulattoes superior to dark Negroes.
1929	Grey, C. T., and Bingham, C. W. (60)	Negroes	258	Seashore (4 tests): Pitch, Intensity, Memory, and Time	
		Whites	219		

- 1929 Peterson and Lanier (129).....Whites.....490
 Negroes.....561
- 1929 Price, J. (134).....Negroes in Neg. Colleges..857
 Whites.....954
 Negroes in White Colleges. 112
- 1929 Young, Paul C. (176).....Negroes.....314
 Whites.....323
- 1925 Garth et al. (38).....Amer. Indians.....1050
- 1926 Fitzgerald and Ludeman (31).....Indians—Mixed and Full-blood.....83
- 1927 Blackwood, B. (7).....Indians.....413
 Mexicans.....200
 Amer. Ind.....256
- 1927 Garth (43).....Amer. Ind.....56
 Whites.....141
- 1927 Garth et al. (44).....Mixed-blood Indians.....765
- 1927 Garth (42).....Nomadic Ind.....215
 Sedentary Indians.....243
- 1927 Garth and Barnard (46).....Full-blood Indians.....170
 Whites.....101
- 1927 Klineberg (82).....Amer. Ind.....120
 Whites.....110
- 1928 Garth and Garrett (47).....Mixed-B. Ind.....942
 Full-B. Ind.....1313
 Whites in P.S.....387
 U.S. Schools f.b.....1032
 m.b.....631
- 1927 Garth (45).....Indians Nomadic.....215
 Sedentary.....243
- 1928 Garth and Isbell (50).....Mixed and Full-blood Indians.....757
- 1928 Garth et al. (51).....Full-blood Indians.....1000
- Northern Negroes more speedy than southern. Nor. Neg. equal whites in Rat'l Learning. Positive correlation between skin color and intelligence.
- Overlapping in Whites 20%; Negro med. 38.8; White med. 49.0. No dif. between Neg. in Neg. colleges and Neg. in White Col.
- Whites excel Neg. in Int. score
 Lighter Neg. excel darker. Neg. more suggestible attitude. Negroes more critical
 Med. I.Q. 68.6, Q. 10.3.
 Med. I.Q. 87
- No reliable difference between Ind. and Mex.
 Indians seem more strongly dextral than Whites.
 No differences.
- Degree $\frac{1}{4}$ I.Q. 77 Correlation between degree. Degree $\frac{1}{2}$ I.Q. 75 of White blood and I.Q. Degree $\frac{3}{4}$ I.Q. 74 with age and educ. constant—42.
 Nomadics 35% better than Sedentary.
- Whites have stronger personalities.
 Indians slower but more accurate.
- Publ. Sch. students slightly superior to U. S. School Indians, but Whites superior to all.
- Nomadic 35% better than sed. on the average.
- No dif. which are significant.
- Med. I.Q. 70. I.Q. rises with school grade. Educ. a strong factor.
- Binet Adap. Rat'l Learn Ment. Maze Disc. Trans. Int. Group Myers Ment. Mus. Tal. Will Temp. Otis Self-Administering
- Adap. of Binet Suggest.
- N.I.T.
- N.I.T. Otis Terman Intern'l Gr. Otis Self Adm. Various observa. Community of ideas N.I.T.
- Psych. Tests Fatigue Downey Group Will-temp. Psych. Tests N.I.T.
- Various Psy. Gr. Tests Seashore Musical Talent Test Otis Classification

TABLE I—Continued

DATE	INVESTIGATORS	RACE	NO. CASES	TEST	RESULTS
1928	Jamieson and Sandiford (77)	Mixed-blood Indians	717	N.I.T. and Non-Lang. Pint. Pat. Perf. Various Psychl. Tests	Mixed-blood Ind. have Non-Lang I.Q. of 97. Perf. I.Q. of 92. Indians and Negroes more accurate but slower.
1928	Klineberg (83)	Indians. Whites. Negroes. Mexican.	284 125 329 1152	Color Prefer.	Sequence R, G, B, V, O, W, Y. I.Q. 78.
1927	Gesche (54)	Mexican.	1004	N.I.T.	Educ. retardations of Mex. 10 mos. Chief cause mental ability; Lang. handicap not serious in mental test. Ave. I.Q. Mex. below Americans. Doubtful interpretation.
1928	Garth (49)	American. Mexicans.	117 119	N.I.T. Pintner Cunningham Myers Pantomime	I.Q. (Binet) 91. Army Beta and Achievement scores equal to Whites. In Personality inferior to Whites. Color sequence: B, R, G, Y, V, O.
1929	Manuel and Wright (104)	Spanish Speaking Mexicans Others English speaking (race not indicated)	128 450 658	Spanish Trans. Reading Stanford Achievement Reading Ex. and English form St.-Binet Army Beta Achieve Personality Color Pref.	Jap. excel. Formosan children. Jap. prefer blue and purple. Formosans Influence of education very strong.
1926	Darsie (20)	Japanese (Amer. born)	1212	Various Psy. tests Color Pref.	A bilingualism is a handicap. Jap. fell below Amer. norm in English tests and below Tokyo norms in Japanese tests (Tokyo norms often higher than Amer. norms). Binet I.Q. 86.6; Army Short I.Q. 91.
1926	Imada (76)	Japanese.	660	Moral and social evaluation of words N.I.T. Eng. form Jap. form	Chinese Med. I.Q. 107.2. Jap. Med. I.Q. 114.2. Chinese rate themselves low.
1926	Kuwata (87)	Japanese. Formosans.	643	Binet Army Short N.I.T.	Inferior to White normals. (Tests need to be adapted to Filipinos.) Italians inferior.
1926	Tanaker, S. (155)	Japanese, boys. girls.	140 98	Mentimeter	
1929	Yoshioka, J. G. (174)	Japanese (America)	38	Pintner Patterson Perform. Personality self-rating	
1926	Graham (58)	Chinese (San Francisco)	73	Haggerty	
1926	Sandiford and Kerr (146)	Chinese.	224	St.-Binet and various	
1927	Traw and Pu (161)	Japanese (Vancouver).	276	Pintner	
1926	Carreon (16)	Chinese.	18	Patterson	
1926	Graham (56)	Filipinos.	665	Healy P.C.I., etc.	
		Italians.	43		
		Jews.	69		
		Americans.	60		

1927	Davies and Hughes (22)	Jews.....	1080	Northumberland	Jews superior.
1929	Garth and Collado (52)	Non-Jews.....	814	Standardized	
		Filipinos.....	1004	Tests in intel. and achievement	Rank of colors: R, G, B, V, O, W, Y. Young Filipinos similar to young of other races.
1927	Wolberg, D. (170)	Jews.....		Color Preference	Jews inferior.
1929	Garrett, H. C. (37)	Non-Jews.....	296	Geometric figures, etc.	
		Jews and others.....		Thorndike Int. test	Jews superior in intelligence and achievement, not in emotional stability.
1927	Mead (109)	Americans.....	160	G. Wash. Soc. Int.	Ital. inferior to Amer. because of language handicap.
1927	Pintner (131)	Italians.....	276	Laird Pers. Inventory	No dif. from Amer. normals.
1929	Wood, M. M. (172)	Belgians.....	100	Otis.....	I.Q., 96.0.
		Bulgarian.....	20	Intel.	I.Q., 95.5.
		Turks.....	26	Non-Lang.	I.Q., 94.3.
		Armenians.....	27	Otis Self-administration	I.Q., 92.6.
		Greeks.....	23		104.
1926	Kirkpatrick (81)	Various Nationalities.....	24	Illinois test	100.
		Americans.....	94	(Army Beta no I.Q.)	87.
		Finns.....	147		In Binet and N.I.T.: 87.2 Chinese highest of all, but Amer. 84.6 Jap. highest in Maze test 84.3 Hawaiian highest in form test. Jews excel Italians.
		French Can.....	158		
		Italians.....	96		
1926	Porteus and Babcock (133)	Chinese.....	212	St.-Binet	
		Japanese.....	229	N.I.T.	
		Hawaiians.....	105	Maze form test	
1926	Siago and Kolden (150)	Portuguese.....	105	N.I.T.	
		Jews.....	276		
		Italians.....	452		
1926	Smith, M. E. W. (151)	Miscel.....	438	Picture test	
		Irish.....	1200	Ohio State Univ. Test	Irish and German more romantic. Italians more classical.
1926	Wang (166)	German } Italian }		Anthropological meas.	Chinese show lang. handicap. Nothing signif. for Russians and Negroes.
		Chinese.....	34	Scholarship	Classified are below. Unclassified are average. "Pure bloods" inferior.
1928	Hayes, E. C. (64)	Russians.....	45	Intel.	
		Negroes.....	158		
1928	Lima (95)	Nordic, Alpine.....	435	Intel. and Reading	Native Amer. excel in intel., but probably due to a language handicap.
1928	Rigg, M. (141)	Mixed group—26 nationalities reported.			
		Native Amer.....	8136		
		Foreign.....	1949		
		German.....	1095		
		Italians.....	1440		
		Bohemians.....	118		
		Jews.....	445		
		Miscel.....	151		
1926	Arthur, G. (5)	I. Immigrant (Surnames) Finns, Russians, and So. Europeans 70 pr. (siblings)		Kulmann Binet	I.Q. of younger higher than older group I.

TABLE I—Continued

DATE	INVESTIGATORS	RACE	No. CASES	TEST	RESULTS
		II. Americans (Surnames) 70			No significant differences group II.
1926	Abell (1)	pr. (siblings)	156	Questionnaires	All intellectual. Some more neurotic than others.
		Polish.....	147		
		Czech.....	119		
		French.....	122		
1926	Goodenough (55)	Americans.....	500	Goodenough Non-Lang. Intelligence	Med. I.Q. 100.3.
		Americans.....	123		Med. I.Q. 91.8.
		Italians.....	456		Med. I.Q. 87.5.
		Sp. Mexicans.....	367		Med. I.Q. 87.2.
		Negro (Cal.).....	67		Med. I.Q. 82.7.
		Negro (South).....	613		Med. I.Q. 76.5.
		Indians.....	79		Med. I.Q. 85.6.
		Jews.....	55		Med. I.Q. 106.3.
1929	Scott (147)	Miscel.....	181	N.I.T.	No difference among whites; Mexicans inferior.
		Amer. born W.....	1731	or St. Achievement	
		Mixed Europ.....	591		
		Germans.....	191		
		Mexicans.....	250		
1926	Hirsch (68)	Polish Jews.....	75	Pintner Cunningham	I.Q. Group
		Swedes.....	232	Dearborn "A" and "C"	102.8.
		English.....	213	Tests largely non-Lang. except-	102.1.
		Russ. Jews.....	627	ing Dearborn "C", which is	100.7.
		Germans.....	190	about 1/2 language	99.5.
		Americans.....	1030	(97% 15 years and less. Lan-	98.5.
		Lithuanians.....	468	guage handicap negligible.)	98.3.
		Irish.....	214		97.4.
		British Can.....	115		95.9.
		Russians.....	90		93.8.
		Poles.....	227		89.6.
		Greeks.....	270		87.8.
		Italians.....	350		85.3.
		French Can.....	243		85.3.
		Negroes.....	449		84.6.
		Portuguese.....	671		82.7.
1927	Frederick, R. (33)	Various students in high school	58	Social Attitude test	Narrow prejudice against other races.
1927	Orata (125)	Whites.....	58	Attitudes toward orientals	College courses make little difference in producing openmindedness
1927	Young (175)	Whites.....	150	Racial attitudes test	Test before and after course on race showed improvement.
1928	Lapierre (89)	French.....	428	Attitudes	French less prejudiced toward colored race.
1928	Thurstone (158)	English.....	315	Rating	Rank on scale: (1) American, (2) English, (3) Scotch, Turks and Negroes last.
1928	Bogardus (11)	Whites, U. S.....	1725	Attitude Relations	A sort of social scale for 40 different races.

primitive groups are not found to be constantly at war with each other; neither do they have perpetual peace in their inter-tribal relations.

(3) *Experimental and Statistical.* First we call the attention of the reader to the tables. Table I shows the array of the studies of the last five years. Table II shows the races studied, the year, and the total number of cases in the span of five years for a racial group, also number of individual studies. Table III indicates the type of test and method used and the year.

The first book embodying experimental results rather exclusively was *Race and Temperament*, by Porteus and Babcock, which appeared in the year 1926 (133). However, many more or less extensive monographs have made their appearance, and this would seem to indicate an importance attached to the subject by investigators. In

TABLE II

GROUPS OF PEOPLES STUDIED AND YEAR

	1925	1926	1927	1928	1929	Total	No. of Cases
Negro (American)	5	6	4	5	5	25	7,158
Indian (American)	1	2	6	5	0	14	5,795
Mexican	0	2	1	3	1	7	4,140
Chinese	0	4	1	0	0	5	561
Japanese	0	6	0	0	1	7	3,311
Hawaiian	0	1	0	0	0	1	105
Jews	0	7	2	1	1	11	1,837
Italians	0	6	1	1	0	8	1,814
English	0	1	1	1	0	3	1,342
German	0	2	0	1	0	3	1,285
Swedes	0	1	0	0	0	1	253
Finns	0	2	0	0	0	2	147
Portuguese	0	2	0	0	0	2	776
French	0	1	0	1	0	2	428
Irish	0	2	0	0	0	2	214
Turks	0	0	0	0	1	1	26
Greeks	0	1	0	0	1	2	293
Bulgarians	0	0	0	0	1	1	20
Filipinos	0	1	0	0	1	2	1,669
Belgians	0	0	1	0	0	1	100
Formosans	0	1	0	0	0	1	643
French Canadians	0	2	0	0	0	2	399
Russians	0	1	0	0	0	1	135
Polish	0	1	0	0	0	1	156
Bohemians	0	0	0	1	0	1	118
Czechs	0	1	0	0	0	1	147
Armenians	0	1	0	0	1	2	150
British Canadians	0	1	0	0	0	1	115
Lithuanians	0	1	0	0	0	1	468
Miscel. (Race not indicated)	2	1	9	3	6	22	3,427
Total	8	57	26	22	19	132	36,882
No. of Individual Studies...	8	19	20	17	9	73	

the last five years no less than ten of these monographs have come from the press.

It is impossible to mention all the experimental studies in detail because of their number, 73. The reader's attention is called to the Tables I and II for detailed information and to the bibliographical references.

The problem just now of prime importance to the investigator seems to be that of differences in intelligence among racial groups. Nevertheless, a great deal of attention is being given to other phases of mental life, some related to intelligence as learning, temperament, musical talent, power of concentration, "mental fatigue," esthetic reaction, color preference, etc. One neglected phase is educational achievement, and this should become a popular field of labor.

Examination of Table II indicates that the period of greatest activity in the last five years was the year 1927. The reviewer can offer no certain explanation of this fact. However, experimentation in race psychology is evidently not languishing, though interest shows some fluctuation.

As to the methods and tests used in experimentation in race psychology one new trend is the rating method, so lightly held in many quarters. The object of the method is to reveal racial attitudes, though this is not always the case because some studies of racial personality have been undertaken by the rating method. It is evident that the method is being more and more perfected so that the results available will be received with more scientific favor. See Thurstone (158).

The performance test, individual, has been more used as has the non-language test in its various forms. A new test of this sort is

TABLE III
SHOWING KINDS OF TESTS, METHOD AND YEAR

Test or Method	Year					Total
	1925	1926	1927	1928	1929	
Individual Intelligence		5	1			6
Group Intelligence	3	16	9	12	9	49
Individual Psychological			2	1	2	5
Group Psychological	2	1	3		3	9
Individ. Performance	2	3	3	1	2	11
Group Non-language		6	3	3	3	15
Group Personal and Will-Temp.....	2	3	1		2	8
Group Esthetic	1	3	4			8
Achievement		1	2	3		6
Group Musical Talent.....				1	1	2
Individual Musical Talent.....				1		1
Attitudes		1	2	3		6

the International Test which is being perfected by Dodds (7). Numerous tests of esthetic reaction are being slightly more used than formerly.

The study of mixed bloods by means of correlation is due to be a very strong trend. One can see its gathering force. This is especially true when we take into consideration the correlation of intelligence and anthropological measurements in the case of mixed bloods. One much needed measurement is that of social status of the mixed bloods; another is correlation between the above mentioned measures and the genealogies of mixed bloods, say of Negroes, Indians, and Mexicans.

It will be seen that the racial group most tested is, as at the time of the last review, the American Negro. Most of the testing has been on the southern Negro, the reason being probably that he is more readily available than the Negro in the north. The figures show that there were 7,158 subjected to various tests. The next most tested group is the American Indian of whom there were tested 5,795. Most of these evidently were those Indian children found in the United States government schools, though a few were found in the common schools. These subjects consisted of full and mixed bloods, though most of them were probably full-blood Indians. The next group in point of numbers tested was the Mexican in the United States. This marks a new trend, for in the period preceding the last five years the Mexican had been little investigated. If one will examine the table, one will see that the Japanese come next. Of course, since figures from the Orient are not readily available, this has little significance.

It must not be supposed that Table I is exhaustive; however, it gives some idea of the number and distribution of the peoples tested in the last five years in experimental studies.

Peterson and Lanier (129) studied for purposes of comparison twelve-year-old northern and southern Negroes in their performance on various intelligence and other tests. They found the northern Negroes superior to the southern Negroes in intelligence test scores, but found both Negro groups inferior to similarly tested twelve year old Whites. In speed of reaction in Rational Learning the Whites excelled the Negroes. Adult groups of Negroes and Whites were tested in musical ability, but the investigators found no evidence of difference satisfactory to them. They found the Downey Will-Temperament Test unsuitable for race work. Coefficients ranging from $-.15$ to $.39$ were found between intelligence scores and various anthropological measurements.

Davenport (21) from results obtained on Negroes in the West Indies concludes that the Negroes are inferior to the Whites tested. Koch and Simmons (84) found rural Negroes and Mexicans inferior to rural Whites. Similar results for the city groups were found. Garth and Whatley (39) tested Negroes of Texas cities, and obtained a group I.Q. of 75. Price (134), testing in Negro colleges, found his subjects inferior to Whites. Davis (23) found for southern Negroes in junior high schools an I.Q. of 78. Young (176) found Louisiana Negroes inferior to his White group in intelligence, and the Negroes more suggestible.

Herskovits (65) found no significant correlation between anthropometric measures of Negroes and intelligence scores. Mercer (111) and Arlitt and Buckner (4) investigated the color preferences of Negroes. Both studies reveal that young Negroes prefer red and blue. Mercer's study showed that the color preference changes with increased education. Gray and Bingham (60) found Whites excelling Negroes in the Musical Talent Test.

Metfessel (112) with the sound photography camera made a study of the folk music of the American Negro. The method is called *phonophotography*, and by it the sound wave produced by the voice of the singer is photographed on a moving picture film by light points. He concludes that present concepts of music of a traditional character are not sufficient for description of folk music, and that many problems of primitive music may now be solved.

Johnson (78) reports that after using the Seashore Musical Talent Tests on 3,500 Negroes, he finds no essential differences in sensory musical capacity of Negroes as compared with Whites. However, he believes that the Negro voice is superior to that of the Whites as respects quality.

Thirteen studies on the American Indian have been reported. Garth and coworkers (44) found group I.Q.'s ranging around 70 for full bloods; and for mixed bloods I.Q.'s increasing slightly with degree of White blood, a correlation of .42. It is shown that the coefficient is larger for the less educated, and smaller for the more educated mixed blood Indians, running from .75 to .20. When comparing Indians in the government schools with those in the public schools the I.Q.'s tend to increase more with education in the former than in the latter for full bloods (47). Klineberg (82) found Indians slower, but more accurate than Whites in speed tests. This he found to be true for Negroes when compared with Whites (82 and 83). Fitzgerald and Ludemann (31) found an I.Q. of 87 for

mixed blood Indians, but the degree of blood was not determined. With a non-language test Jamieson and Sandiford (77) found for a large group of mixed bloods an I.Q. of 97.

Garth and Barnard (46) administered the Downey Will-Temperament Test to full blood Indians and Whites. While their measured results show great variability, they draw some conclusions, one of which is that on the whole, in the light of the interpretation of the author of the test, that Whites appear to have the stronger personalities.

Garth and Isbell (50) studied the musical talent of full and mixed blood Indians with the Seashore Test. They interpret the data as indicating no racial differences in musical talent.

Blackwood (7) administered the International Group Mental Test to Indians and Mexicans, but found no significant difference between the scores of these peoples.

Downey (26) in tests for handedness found Italians more strongly dextral than Whites.

Garth (49) reports a median N.I.T.-I.Q. of 78 for a group of 1,004 Mexicans. Garretson (36) using various non-language tests found the Mexicans below the performance of Americans. Good-enough (55) with a non-language test found for a group of 367 Spanish Mexicans an I.Q. of 87. See also Koch and Simmons (84).

Gesche (54) tested over a thousand Mexicans for color preference, and found that while Mexicans and other peoples are similar in their color preferences when they are young, education makes a difference, and probably follows tribal preferences. For the total group, 1,152, she found the Mexicans' color preferences to be as ranked: Red, green, blue, violet, orange, white, and yellow.

The most important study of Japanese in the United States was on school children by Darsie (20). In a group of 658 he obtained results indicating an average Binet I.Q. of 91. With the Army Beta he obtained measures of the Japanese children equal to that of the Whites. This investigator concluded that if allowance be made for the language handicap the Japanese would have an I.Q. around 99 or 100. Yoshioka (174) studied Japanese children in the United States likewise to ascertain the effect of bilingualism. He used the National Intelligence Test, and a Japanese translation of the same on thirty-eight children. The children tested fell below the White norms in the United States, and Japanese norms obtained with the N.I.T. in Tokyo. His conclusion is that bilingualism is a handicap to the child. Kuwata (87) reports the comparison of 660 Japanese and 643

Formosans in their performance of psychological tests. The Japanese children excel the Formosan children. Kuwata also administered a color preference test. The Japanese preferred blue and the Formosans preferred red.

Imada (76) administered a color preference test to 1,212 Japanese in Japan. The color preferences in order are: Blue, red, green, yellow, violet, orange for the group.

Graham (58) obtained a Binet I.Q. of 86.6 for a small group of Chinese, 73, in San Francisco. Sandiford and Kerr (146), using over two hundred each of Chinese and Japanese subjects (Vancouver), obtained with the Pintner-Patterson Performance Test a median I.Q. of 107.2 for the Chinese and 114.2 for the Japanese.

Very striking results have been secured from testing immigrant groups. Arthur (5) found in testing siblings of immigrant families that the younger children had higher I.Q.'s than their older sibs. The 92 pairs of sibs were from families of Finns, Russians, and southern Europeans. Hirsch (68) tested large groups of immigrant children. The highest I.Q.'s were obtained by Polish Jews, Swedes, English, Russian Jews, Germans, Armenians, Lithuanians; beginning with 102.8 and ending with 97.4 I.Q. The lowest average I.Q.'s was obtained by the Portuguese group which was composed of 671 individuals. The results are interesting since the tests were largely non-language tests.

Pintner (131) in testing Belgians in Belgium found norms no different from American norms for his non-language test. Carreon (16) administered the Haggerty Test to Filipinos with no conclusive results. Porteus and Babcock (133) testing in Hawaii with the Stanford Binet and other tests found the Chinese with highest I.Q.'s in comparison with Japanese, Hawaiians, and Portuguese. The Hawaiian boys were highest in the form test. Wood (172) tested Bulgarians, Turks, Armenians, and others in Constantinople with the Otis Self-Administering Intelligence Test. The Bulgarian group was shown to have the highest I.Q.—96, the Turks were next with an I.Q. of 95.5. The Armenians and Greeks received I.Q.'s of 94 and 92 respectively.

Garrett (37) tested Jews and others with Thorndike Intelligence Test, George Washington Social Intelligence Test and Laird Personal Inventory Test. The Jews, he reports, are superior in intelligence and achievement, but not in emotional stability. Wolberg (170) in comparing Jews and non-Jews with recognition of geometrical figures found the Jews to rank lower than the non-Jews. In Great

Britain Davies and Hughes tested Jews and non-Jews with the Northumberland Standardized Tests in Intelligence and Achievement. The results showed the Jews to be superior to the other group. Graham (56) administered the Stanford Binet and various performance tests to Italians, Jews, and Americans. In this study the Italians were found to be inferior to the others.

Garth and Collado (52) made a study of the color preferences of 1,004 Filipino children, mostly full bloods. They find that young Filipinos are quite similar to the young of other races in their color preferences, but that tribal or other nurtural influences change this early more or less native color preference. The scale for the total group of Filipino children shows a sequence running from most preferred to least preferred with some confusion in preference along the way, of red, green, blue, violet, orange, white, and yellow.

Mead (109) found Italians inferior to Americans but thought this was due to language handicap. Likewise Rigg (141), studying various foreign groups, as Germans, Jews, Italians, Bohemians, found them inferior to the American in performance, but concludes that this is due to language difficulty likewise. Smith (151) tested Irish, Germans, and Italians with a picture test, and concluded that the Irish and Germans are more romantic than the Italians, but that the latter are the most classical.

Thurstone (158) sought to get a scale for preferences by means of rating. He used 239 judges; the rank was first, Americans, then English, Scotch, etc. Lapiere (89), using an attitudes test, concluded that French are less prejudiced toward colored peoples than English. Frederick (33) and Orata (125) after investigations with attitudes tests found their subjects possess narrow prejudices upon which courses in college, as anthropology, sociology, make little impression. Young (175) found the situation improved after such college courses, by testing before and after the courses. Bogardus (11) obtained a sort of scale for measuring social distance for forty different races as determined by over 1,700 judges, all White Americans.

Several summaries and digests of experimental findings on racial differences have appeared in the last five years. We call the reader's attention to summaries of published results found in introductions to actual studies (*Cf.* Peterson and Lanier, Koch and Simmons, Klineberg, and Blackwood). In reviews such as Garth's (40 and 48) accounts of the results of testing for racial minds have appeared. Among the criticisms and evaluations of the testing we refer first to the trenchant article by Viteles (162) who surveys the results of the

studies of the Negro. He calls attention to the differences found between northern and southern Negroes, and to the influence of education as producing differences, to the lack of opportunity of the Negro, to the fact that Whites in some sections do not do so well as Negroes in other sections. Thompson (157) of Howard University makes a similar survey, and asks us to note that possibly segregation in our public schools causes much of the differences found between Whites and Negroes. Gregg (61) in interpreting the results of findings on Negroes insists that having a White experimenter is a handicap to the subject being tested. All these three students are of the opinion that education and selection are the causes of the differences found. In the opinion of the reviewer these three studies should be very carefully considered by all thoughtful readers.

Hsaio (71) has gathered into one compact system results of practically all studies of psychological experimentation on Chinese and Japanese. It makes a very valuable survey. He shows that Chinese and likewise Japanese, when proper consideration is made of language handicap, are about the equals of Americans in results obtained by the Stanford Binet. In other traits besides intelligence it may be different, as in rote memory where the Chinese are better. He emphasizes the importance of social status, the failure to adapt the tests to racial ways of thinking, and the neglect of variability.

In order to get some notion of the status of opinion held by representative psychologists we have listed in our bibliography several recent textbooks. We note that out of seven representative texts, all admonish caution in the matter of conclusions to be drawn from present experimental studies, though some appear to be of the opinion that racial differences in mental traits exist. Cf. Woodworth (171), Hunter (74), Freeman (34), Sandiford (145), Gates (53), Jordan (80), Ellis (27).

Conclusion. What then shall we say, after surveying the literature of the last five years, is the status of the racial difference hypothesis? It would appear that it is no nearer being established than it was five years ago. In fact many psychologists seem practically ready for another, the hypothesis of racial equality. But the problem in either case is the same as it was—to obtain fair samplings of the races in question, to control the factor of nurture, and to secure a testing device and technique fair to the races compared.

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THEORY AND MEASUREMENT OF ATTITUDES AND OPINIONS

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One of the most important (and confused) subjects in the interlocking and overlapping fields of sociology and social psychology is the category of motivation. It is apparent that human movements are possible only when appropriate action-patterns exist and that these patterns must be either inherited or acquired. It is also apparent that the functioning of both human and non-human animals is largely motivated by action-patterns that seem to be products of germinal development. These patterns are present at birth or soon after, relatively stable and unmodifiable, common to the species, usually adaptive and largely unlearned. If they are relatively simple, like grasping, knee-jerking, winking, they are "reflexes"; if they are more complex like crying, suckling-swallowing, assimilating-excreting, manipulating, they are "instincts." Just how complex such responses must be to be "instincts" is undetermined and perhaps indeterminable. The safest procedure, probably, is to call them instinctive, innate, native, germinal, and let it go at that.

On the other hand, it is equally obvious that most human cultural behavior is motivated by action-patterns acquired through social conditioning. It is his greater capacity for building new action-patterns that distinguishes the human animal from the non-human animal. Following Thorndike, it is customary to describe the innate type of behavior as *original* and the second type as *acquired*. Thus, while vocalizing is an original trait of man, speaking a language is an acquired trait; hunger, putting-objects-in-mouth-and-swallowing, is natural, but seeking specific foods is a culture-trait; sex activity is native, but particular love-making and marriage-family complexes are acquired action-patterns. The most significant *human* acquisitions are the action-patterns we call language, vocal symbolization, thinking, "consciousness." These habits, like all other action-patterns, are neuromuscular mechanisms. Thinking is acting; "consciousness" is movement of specific parts of the organism.

Since the discovery of the "conditioned reflex," it has been found that stimuli capable of activating these neuromuscular mechanisms, whether innate or acquired, are very numerous and very variable. Under appropriate conditions, almost any stimulus may be substituted for the stimulus originally adequate to activate any given action-pattern. Then a substitute for the substitute stimulus may be supplied, and so on almost indefinitely. While the functional motor-mechanisms, innate or acquired, are relatively stable, as in the Watsonian infantile fear, love, and rage responses (117), many quite diverse stimuli may be integrated with these specific responses. The result is a new total stimulus-response action-pattern which is partly original, but which in most *human* behavior, is largely the result of experiential conditioning in cultural situations.

Hence, the attempt to ascribe human behavior to specific "instincts" has failed. Modern sociologists and social psychologists are studying acquired and conditioned behavior intensively in the hope of achieving some scientific understanding of human, as contrasted to human-animal, motivation. This point of view is expressed in the proposition that "human behavior is cultural behavior." The specific-instinct explanation of much animal behavior is demonstrably false and some observers even allege traces of cultural behavior among animals. Certainly it is true that much adjustment-behavior of animals is conditioned, learned, or acquired, whether it be cultural or not in the sense of being transmitted by some process of symbolical communication or training in group-patterned responses. In any case, it is clear that no adequate understanding of human behavior is possible on the old specific instinct hypothesis. The problem of human motivation is the problem of acquired responses. We may grant an "instinctive" substrate for all such behavior, but scientific explanation must be made on the level of the substitute-stimulus integration. In a given human adjustment it is practically impossible to unscramble the innate action-patterns from the innate conditioned ones. A conditioned action-pattern is as much a "drive," is as dynamic a motivation, as a raw instinct. When the conditioned integration is once achieved, it responds automatically upon the presentation of the appropriate stimulus. It is as much an integral and organic part of the functional organism as if it were an original, innate stimulus-response pattern. This is particularly true of conditioned response patterns of the unstriped muscles (Watson, 50, 51), but it is also true of all so-called voluntary habits. In language habits, this process is especially rich and varied and is of especial interest to

students of human behavior, since so much of our actual social adjustment is mediated by language habits.

The purpose of this paper is to present a bibliography of some of the material that has appeared since 1925 on the general subject of acquired motivation-patterns. A few basic studies prior to 1925 are included. A rough classification has been attempted. This serves the two-fold purpose of giving some guidance to the student and of simplifying the short discussion that follows. Needless to say, the bibliography is incomplete, but the compiler believes it will serve as a fairly adequate introduction to the subject. He urges the reader to place but little confidence in the too brief analysis that follows, but rather to consult the literature.

The bibliography is limited chiefly to the writings of sociologists and social psychologists. A great deal of similar or allied material will be found under appropriate headings in the *Psychological Index* since 1925. The compilation is mainly concerned with the theory, observation and measurement of "attitudes and opinions" as used by sociologists and social psychologists. No one realizes more vividly than the compiler himself that these are very ill-defined terms, but he believes that they do refer in a general way to types of acquired action-patterns that are definitely concerned with human motivation. Certainly, "attitude" is not more vague and ill-defined than "trait," as Prince (38) and G. W. Allport (2) have shown. While it must be confessed that most writers use such terms as attitude, trait, opinion, wish, interest, disposition, desire, bias, preference, prejudice, will, sentiment, motive, objective, goal, idea, ideal, emotion, and even instinct and reflex, loosely, indefinitely, and often interchangeably, yet it must also be admitted that there is a core of common meaning in all such usages. These, and other similar terms, refer to acquired and conditioned action-patterns that motivate human social behavior. If this is kept clearly in mind, much of the terminological discussion will appear as "sound and fury signifying nothing" more than a mere symptom of the "disease of language." Men who have waged epic verbal battles often discover later that they were talking about the same thing. Common usage is necessary, to be sure, if a valid scientific vocabulary is to be won, and perhaps this can be achieved only by bloodless verbal combat. But intensive research into the repetitive uniformities of acquired motivation-patterns will do more toward clarifying and standardizing terminology than will the making of many violent verbal assaults and lengthy language-defenses. The literature listed below shows that the technique and theory of such

research is already well advanced. The various investigators are studying the same kind of data, whatever they call it, viz.: socially conditioned patterns of motivation. It should be pointed out that the more careful writers attempt to give clear and unmistakable meanings to the terms they use. Such procedure will eventually settle the terminology difficulty.

Symonds (45, 46) has mentioned seven common meanings of the term attitude: (1) great organic drives, purposes, motives; (2) muscular adjustment; (3) generalized conduct; (4) neural set or readiness to adjust; (5) emotional responses; (6) feelings; (7) verbal accepting or rejecting responses. The "tendency to act," positive or negative, of Thomas (48) and his followers, Park and Burgess (32), Faris (18, 19), Bogardus (8), House (24), Lundberg (70), *et al.*, seems to be quite similar to (4), above. This is perhaps the most prevalent use of attitude among sociologists, although we should add the preparatory movement or partial adjustment idea of Bernard (5), the relatively stable overt status-getting response of Bain (3), and the trenchant criticism of all the above by Markey (28) with his conclusion that attitudes are behavior integrations associated with signs and symbols of probable behavior.

Most attempts to study attitudes have been by way of getting verbal responses through questionnaires, rating of verbal symbols in gradations of liking-disliking, and asking people for preferences, desires or interests. This material is roughly classified under section V. The assumption seems to be that people really do, or will do, what they say they have done, or will do. The psychoanalytic school has shown the falsity of this assumption. (It should be noted in passing that there is a rich psychoanalytic and psychiatric literature on the general subject of motivation which is not touched upon in this compilation. A special bibliographic review of psychiatric attitude theory and study should be available.) Some students have attempted to determine the degree of correlation between verbal responses and actual behavior, *e.g.*, Zimmerman (119), May and Hartshorne (99, 100), and Terman (110). Stouffer (79a) has found a correlation of .96 in the judgments of attitudes from case studies (unpublished as yet). Several persons read the same case studies and listed the attitudes they thought were therein exhibited with the high degree of agreement indicated above. If this work stands the test of verification, it will be highly significant. Practically all investigators, when pressed, will admit the probable discrepancy between verbal and actual behavior, especially if the verbal "attitudes" are on

tabooed subjects as many of them are. An enormous amount of time and labor have been practically wasted as a result of this naïve assumption that verbal statements are highly correlated with adjustment behavior. That there is often high correlation must be admitted but it must be scientifically (quantitatively) determined just what this correlation is if the resultant generalizations are to have any scientific (predictive) value. Symonds (45) and Bain (3) have argued that verbal "attitudes" are relatively unimportant, but it should be pointed out that for *some problems* the verbal response is what is desired. However, in most cases, the assumption is that the verbal response is indicative of actual behavior, as buying, voting, church going, choosing occupations, etc. This may be so, but it must be *determined*, not assumed, before the study has any great value. Practically all writers explicitly or implicitly admit a distinction between "attitudes," however used, and "verbal attitudes," or opinions.

Russell (41) has approached the problem of motivation by way of desire and feeling (by which he seems to mean about the same as attitude), while Holt (23) has emphasized the structural nature of the "wish." These two approaches seem to be the rationale back of the two types of attitude studies I have classified as Overt (III) and Verbal (V). The only way we can tell how another person "feels" is to observe his behavior and assume that he "feels" that way, or "desires" those responses, or else to ask him how he "feels." The one approach results in objective analysis, the other in subjective. Faris (18, 19), following Thomas (47, 48), holds that "the attitude" is the result, largely, of crisis situations, while the "wish" is the precursor of action. Thus Bernard's attitude (5) is Faris' wish, while Holt's wish is more nearly like Semon's "engram" or Pareto's "residue." Faris insists upon the necessity for studying the "subjective aspect of culture" (17) by imaginative in-living, *Einfühlung*, somewhat after the model of Cooley's "sympathetic insight" or Weber's "*verständliche Soziologie*" and "*Nacherlebbarkeit*." Faris admits a probable structural basis of attitudes, but insists that the adjustment aspect of attitude is the datum of social psychology and as such is much more important than the hypothetical engram (18). With this, most would agree heartily.

The *Gestaltists* make frequent use of "attitude," yet the closest Köhler (27) comes to a definition is: "A change of attitude involves a definite physiological stress exerted upon a sensory field by processes originating in other parts of the nervous system" (p. 184).

Attitudes are directed toward or away from objects (pp. 323 ff). He speaks of analytical, emotional, indifferent, dynamical, and introspective attitudes. It is difficult for Köhler to understand how a "directed attitude" depends upon a definite physiological situation of the organism as is the case in hunger or sex, but he thinks it does.

Space precludes further theoretical discussion. The reader is referred to Section I, and especially to the bibliographic articles of K. Young (56), Bernard (5), House (24, 25), Roback (39), Faris (20), G. W. and F. H. Allport (58), G. W. Allport (2), Lundberg (70). The whole subject of motivation is ably presented and criticized by Sorokin (44).

Practically all investigators agree that human motivation is largely in the realm of habit, even though these habits may be merely conditioned original action-patterns. However, the native part of the integration is important only as a substrate upon which the superstructure of habitudinal attitudes or traits may be built: Dewey (15), Bernard (5), G. W. Allport (1, 2), Prince (38), Hart (22), Watson (51), *et al.* The significant thing is that there is considerable stability and cultural uniformity in these adjustment responses. This leads to the attempt to measure. All measurement, and hence all exact science, depends upon relative stability and uniformity in the behavior of defined units. All units, however carefully defined, are variables. The problem of measurement is to find sufficient stability and uniformity among reciprocal variables regarded as (hypothetical) identical units so that mathematical statements (or generalizations) of this uniformity may be made. Such generalizations constitute scientific description and explanation. Phenomena are "explained" scientifically when their uniformities are mathematically determined. Then prediction is possible. Thus, all accurate observation that does not result in generalizations of prediction is merely preliminary to scientific explanation. Hence, a great deal of what passes for "scientific fact" and "scientific knowledge," by this criterion would be merely hypothesis.

It has proved to be a very fascinating, though difficult, mathematical problem to define the units and devise the techniques of measuring such complex variables as the types of behavior herein discussed. But it has not been insuperable. The references in Section II describe some of the more successful efforts. The techniques are in general the same as those long ago worked out by the biometric school and the "intelligence" testers, but some new corre-

lation formulas have been devised and the methods of multiple and partial correlation are being found increasingly necessary.

As indicated above, the two possible approaches are to study (1) overt behavior and (2) verbal responses. It is obvious that the latter is the easier. The questionnaire device is ample proof. There are two intermediate types of study between overt and verbal responses. One is to ask the subject to check from a list of specific acts those he has performed within a definite time. This is exemplified by the play studies of Lehman (210) and of Lehman and Witty (104, 105, 118). The other is the case study method: Shaw (158), Bogardus (126), Burgess (132, 133), Healy (139, 140), Kreuger (143, 144), *Social Forces* (159), *et al.*, found in Section IV. In the first type, if the time interval is not too great, and the subjects are honest, and the acts accurately defined, actual adjustment behavior may be determined. In the second method, the verbal responses may be evaluated by the skilled interviewer, and the statements verified later by actual factual investigation. But such materials need to be treated statistically before scientific generalizations can be made, as Shaw (158) and Bogardus (60) have shown. Bain (122) and Perry (34) and others have criticized life-history, case study and questionnaire methodology.

The attempts to measure opinions, or verbal attitudes, range all the way from the simple summation of judgments of true and false statements to very elaborate attempts to construct a rational scale of equal steps. Thurstone has developed the latter method to its highest point in a series of brilliant papers (81-84). His is really an elaboration and logical improvement on the earlier rank-order scales of F. H. Allport (168), Rice (109), and others. Hartshorne and May (64) constructed a scale of equal steps in terms of standard deviation on a normal curve. The fault of this method lies in the assumption that the opinions comprise a normal distribution as K. Young (54, 56) has shown. Thurstone's method, by several ingenious devices, succeeds in establishing a statistically valid scale of equal steps. But it is very laborious and costly. In one study (84) he had 200 persons sort Allport's questions and states that there should be 500 or 600! It appears fairly easy to construct a reliable test of verbal opinions but the problem of validation is more difficult. This commonly rests upon the judgment of raters in the last analysis, and they seldom agree closely, as Kornhauser (66, 67, 68) has shown. However, Thurstone's method seems to be the best so far devised.

G. W. Allport has suggested a method of validation that dispenses with raters (78).

The social distance studies of Bogardus (7, 9, 11, 60, 173-177, 245), Poole (36, 37), Shideler (79), Woolston (234), *et al.*, are merely variations of the rating method, though more informative than mere likes and dislikes. The extroversion-introversion tests depend upon self-rating, perhaps checked by interviews, as with Hewlett and Lester (65), or by taking the extreme statements as judged by raters (Conklin, 62).

In addition to the discussions of theory and technique of measurement by Thurstone, Rice, F. H. Allport, Hartshorne and May, mentioned above, one should consult the bibliographic reviews of measurement compiled by F. H. and G. W. Allport (58), Manson (71, 72), May and Hartshorne (73, 74), G. B. Watson (86), and the books by May and Hartshorne (99, 100), Rice (76, 109), Lundberg (70), Meltzer (75), and Terman (110), and the very valuable theoretical discussion by Kirkpatrick (65a). Good discussions of the shortcomings and dangers of statistical analysis are those of Cooley (135, 136), Lehman and Witty (69), and Burgess (132).

May and Hartshorne (74), in the best discussion of tests, both as to classification and methodology, point out that tests seldom duplicate life situations. In an attempt to approximate this some studies have been made of actual overt adjustment behavior under controlled or uncontrolled situations. J. B. Watson (51, 117) was one of the pioneers in this field. Voelker (115) in an admirable study of dishonesty was another. May and Hartshorne in the studies mentioned have perhaps done the most notable work in this field. Blanchard and Paynter (242-244), Carmichael (93), Chapin (95), M. C. Jones (103), Lundberg (106, 107), D. S. Thomas *et al.* (111), Zimmerman (119), Rice (109), and others cited in Section III should be consulted. It is interesting to note that many of these studies are of the adjustment behavior of infants whose verbal responses are not highly developed.

It is also noteworthy that many of the studies in Section VI have approached the question of changing attitudes by observing actual behavior, *e.g.*, Blanchard and Paynter (242-244), Hershey (248), M. C. Jones (249), Watson (256), Bogardus (245), Zeleny (261), Marston (251), Pearson (253), Tanquist (255). Verbal attitudes remain quite constant according to D. Young (259), Willey and Rice (258), and Rice (222). When verbal stereotypes are highly developed, they may remain intact, even after overt adjustment atti-

tudes have changed. A nice theoretical explanation of this is offered by Pitkin (153, pp. 316-333) by pointing out the differing numerical magnitudes of cortical and muscular patterns. This is perhaps the structural basis of many "personality conflicts."

The materials in Sections III and VI are somewhat similar in that overt behavior tends to be the subject matter of both. Sections IV and V are also similar, both being based largely on verbal response. However, case study (IV) is somewhat intermediate because the verbal reports are "interpreted" more or less objectively by the investigator and also often checked by actual non-verbal behavior. A variety of case study dealing with the group or small institution, as advocated by Cooley (135, 136) and Bogardus (127), is illustrated by Angell (120), McClenahan (150), Lynd (147), Zorbaugh (166), and Rhyne (154). This method is really a combination of the methods of verbal response, *Einfühlung*, statistical tabulation and observation of overt behavior. It promises to increase in importance. Most of the life-history and case-study research, however, deals with individuals, usually maladjusted. This work is obviously more valuable for social art than for the science of sociology. Such generalizations as are valid are applicable only to relatively small classes of people. When inferences from such studies are made regarding the behavior of normally adjusted people, the results are likely to be quite dubious.

Most of the studies in Section V are mere tabulations of verbal responses, usually likes and dislikes, true and false, or preferences, though some of them are quite elaborately treated statistically. The fundamental work of F. H. Allport, Thurstone and Rice mentioned above is largely based on verbal responses, although Rice is concerned with more overt behavior in his studies of voting. One might argue that voting is really verbal whether one does it on a party ballot or on a rating scale. Yet it is one thing to study the election returns and quite another to ask people why they voted as they did or how they would vote for so and so and such and such. Political voting is much more nearly overt adjustment behavior than is "voting" on a classroom questionnaire.

While no comprehensive logical classification of the subject-matter covered by the attitude studies listed below is attempted here, certain types may be pointed out. Space prohibits placing all the studies under appropriate headings even if that were logically possible. I have merely tried to indicate certain types of attitudes, traits, or interests that have been studied by several investigators. Definite

subdivisions under some of the headings could be made on the basis of studies already completed, as in "race and national," "vocational," "delinquency," etc., and eventually this will be necessary as the number and variety of such studies increase.

1. *Racial and national attitudes:* Arai (121), Lasker (145), D. Young (259), P. V. Young (239), K. Young (57), Garrett (188), Bogardus (126, 128, 173, 177), Ravitch (218), Gibson (189), Lapierre (208), Woolston (234), Sunne (227), Reinhardt (219), Wu (235), W. I. Thomas (48), G. B. Johnson (102), Lehman and Witty (104).

2. *Play and recreation attitudes:* Lehman and Witty (105, 118), Verry (114), Alderson (167), Hurd (200), Lehman (210), Stoke and Cline (224).

3. *Attitudes of family and children:* Blanchard and Paynter (91, 242-244), L. G. Brown (131), M. C. Jones (103, 249), Verry (113), Walker (116), Burgess (133), McChristie (148, 149), Nimkoff (151, 215), Sayles (155, 156), M. E. Watson (163), J. B. Watson and Raynor (117), Wembridge (164), Fenton (185), Gross (247), Rice (221), Wickman (232), Pearson (253), Zeleny (261).

4. *Vocational interests and attitudes:* W. A. Anderson (170), K. M. B. Bridges (92), Brotemarkle (180), Cowdery (183), Freyd (186), Pickett (216), Remmers (220), Lind (146), Hubbard (197), Strong (225, 226), Wilkinson (233), Zimmerman (119).

5. *Political attitudes:* G. W. Allport (169), F. H. Allport and Hartman (168), Lasswell (250), Lundberg (106, 107), Moore (213), Willey and Rice (258), Rice (109).

6. *Religious attitudes:* Bain (171), Calkins (134), Sheldon (42), J. R. Young (238), Zimmerman and C. A. Anderson (89).

7. *Extroversion-introversion:* Conklin (62, 182), Guthrie (191), Heidbreder (194, 195, 196), Hewlett and Lester (65), Jasper (202), Marston (251).

8. *Delinquency attitudes:* Balch (124), Brietz (130), Fisher (137), Guilford (190), Haggerty (192), Healy (139), Healy and Bronner (140), Sayles (155, 156), Shaw (157), Sullenger (160), TeWater (162), Wembridge (164), P. V. and E. F. Young (165).

9. *Honesty and right and wrong attitudes:* Brogan (178), Carmichael (93), Chambers (94), Hartshorne and May (98, 99, 100), V. Jones (204), Terman (110), Voelker (115), G. B. Watson (231).

10. *Composite, i.e., attitudes on various questions all confined in the same schedule, e.g.,* Brogan (178), Davis (184), Harper (63),

Hart (193), Jordan (205), Katz (206), Meltzer (75), Neumann (214), Sturges (254), Wyman (236), Zeleny (240), *et al.*

Many other interesting studies of the attitudes involved in specific personality traits such as G. W. Allport on ascendance-submission (59), J. W. Bridges on college emotional instability (61), Hawthorn, rural-urban personality (101), Morris, mendicancy (108), Hershey, periodic emotional changes (248), and studies of attitudes involved in certain typical social relations such as Binnewies on community spirit (90), Chapin on volume of social stimuli (95), Clark on student interviews (96), Vance on cotton culture (112), E. F. Guthrie on personality crises (138), Tallman on college self-government (161), G. V. Brown on economic self-government (246), and others, should be mentioned.

In conclusion, we may admit that the measurement of what G. B. Watson calls "the less tangible qualities" (85) has shown remarkable development in the last ten years, and at the same time, insist that much more remains to be done, both in theory and practice. Many of the present scales are quite reliable but how valid they are is a matter of doubt. Most of them have been validated by the rating method and Kornhauser's study of the fallibility of raters (66, 67, 68) merely confirms common experience. Thurstone's method of validation by rating, though costly and laborious, is the best one so far devised. Kornhauser's suggestion of trained raters might simplify the procedure somewhat and G. W. Allport's suggestion for dispensing with raters altogether (78) may prove feasible. Mathews (212) has shown that the order of the questions on a schedule produces a marked effect upon the nature of the replies, in some cases as high as 7 per cent. It is probable that the personality of investigator, the artificial social situation of the class-room, the time of day, condition of the weather, indifference or enthusiasm of subjects, imminence of exciting events such as football games, junior proms, etc., would also affect the results of verbal response attitude studies.

I think it is safe to say that the bulk of the work so far done has little value except to show how not to do it, to define difficulties, and thus indirectly to advance theory and technique. Most of the studies are chiefly fallible, I think, because they do not duplicate life situations (*Cf.* May and Hartshorne (74)). The remedy would seem to be this: Attitude studies should be based upon actual adjustment behavior (Zimmerman *et al.*, 88) and correlations of verbal and overt behavior. If significant correlations are found, then, for certain types of attitudes characteristic of certain classes of people, the verbal atti-

tudes may have predictive value for actual adjustment behavior. Even if we find high uniformity in the verbal responses of a group at different times, or of different groups at the same time, we have no surety that a similar uniformity will be found between their verbal and adjustment behavior, especially when tabooed or ill-organized behavior is the subject of investigation. "Attitudes" that do not motivate adjustment (or maladjustment) have little significance for human beings.

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RECENT SOCIAL PSYCHOLOGY

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Social psychology to-day is largely a product of American scholarship and research. American politics, philanthropy, industry, jurisprudence, education, and religion have demanded a science of control and prediction of human behavior, not required by similar but less dynamic institutional counterparts in other countries. The net result is a mass of literature representing a vast field of inquiry and speculation, bearing tangential relations to all the social sciences. No student knows where to draw the line between social psychology as such and a score of other special sciences, which have to do with group action, interaction, and reaction, human and infra-human in reference.

The present brief digest represents an attempt to indicate the trend of progress in social psychology during the past two years. The bibliographical citations are by no means exhaustive, or even completely representative, for the by-paths of this pioneering are so numerous that a complete survey of its connections with other branches of social science would require the coöperative efforts of several specialists.

The Evolution of "Pattern" Psychology. Social psychology as a science is still in the making. It has yet to earn an undisputed place among the social sciences. In order to grasp the meaning of present trends, one must first briefly recount its progress during the present century. Assuming, as some have, that McDougall is the founder of social psychology, we may note that his first text (69) emancipated the growing science from the mysticism of Durkheim, Espinas, Tarde, and earlier students, even though it substituted one brand of mysticism for another.

The earlier students emphasized the group as the unit of social science. Very generally the group, mob, race, club, nation, sect, or what not, acted as a coercive agent. The basic question was: What happens when an individual becomes a member of a group? McDougall looked to native forces within the individual, which may on occasion become operative. In the interest of specificity of

explanatory causation he forced his reasoning into compartments which could not be squared with the facts of individual variability and social progress. But he did narrow the field.

It remained for F. H. Allport (2) to show in a systematic way that the nature of the individual is vastly more variable than the instinct theory as such can account for; that social situations, even the highly fortuitous so far as organization is concerned, are equally important as determiners of social behavior; that a "give and take" relation exists between and among individuals and their respective environmental situations and preconditions. For present historical perspectives we may say that social psychology to-day is progressing in line with that psychological mechanism called the conditioned reflex, popularized by the behaviorists, ably and succinctly applied to the field of pure sociology by Thomas (85).

Some conditioned reflex patterns seem to certain students to fall into the category of inherited predispositions. Thus Bartlett (6) argues that vocational temperaments appear as predispositions similar to Allport's "prepotent habits"; that persons born in a certain social class are predisposed toward certain occupations, skills, and general outlook upon social life. Loukas (58) studied the social consciousness among university students and concluded that such patterns as friendships are acquired on the basis of similarity of ideas, morals, and interest in amusements. The force of environment is best shown by Hayner (44), who studied the effects of hotel life on the development of personality patterns. Burgess (15), as editor, has brought together a valuable collection of papers, which perhaps better than any single contemporary volume portrays the importance of pattern psychology as a categorical approach to the understanding of psychosocial phenomena. This collection comes close to qualifying as a textbook for undergraduates.

It would appear that social psychology in America began in 1924. It is an outgrowth of a controversy, which as such well illustrates one of the principles of any evolving science (12). Out of this controversy has come a settled conviction that patterns as matters of individual acquisition will explain all psychological phenomena, social and individual. As investigation proceeds, the once widely accepted notion that individual psychology is one thing, and social psychology another, has found a place in the scrapheap of exploded psychological presuppositions. One is inclined to agree with Walther (95), who insists that the so-called behavior pattern should take first place

among the research problems of social psychology. This he regards as the long-sought "entity" of the science.

Structural Psychology and Functional Psychology. It is possible to study social psychology from the point of view of cross-sections of behavior (structural psychology) on the one hand, and from the point of view of synthetic integrations (functional psychology) on the other. These methods (or investigatorial attitudes) are really complementary rather than antithetical, as Markey (66) points out.

Those who hold that, methodologically, scientific facts may be classified according to their "reach," are not adverse to an interpretation of social behavior in terms of whole-body movements in response to whole-environmental situations. A given student may choose almost any formulation of the pattern psychology he wishes and still be within the range of legitimate scientific inquiry.

It is precisely on this methodological point that Markey finds his "frames of reference," which denote the difference between psychology and sociology. Any psychologist may be expected to study his data in terms of the tradition established by Allport, while the sociologists may be expected to interpret social behavior in terms of Hankins' (40) suggestion that "In spite of the enormous potential variations in habit, motor, emotional and intellectual structure, each individual selects, within limits, those stimuli to which he will respond, responds to them in his own way, and thus evolves a personality which represents the response of his particular neural system to his own world."

It should, therefore, be said that the distinction between structural psychology and functional psychology to-day amounts to little more than academic verbiage. In this connection Young (102) anticipates the methodological future in making a plea for a combination of what he terms historico-genetic and anatomical-statistical procedures.

Personality. While for the most part the earlier literature on social psychology is speculative or at best merely inferential, the yield of the last few years betrays a decided interest in experimentation by way of the case history method. It is no exaggeration to say that psychology has been substantially reconstructed during the past fifteen years. This reconstruction has resulted in new and better concepts of individuality developing into personality. While most students (psychiatrists excepted), even to-day, are unwilling to pay more than passing obeisance to Freud and his psychological progeny, mostly wayward and incorrigible, the fact remains that psychoanalysis in one or another of its various ramifications qualifies as a theoretical

articulation point between structuralism and functionalism. We owe more, as a matter of motivation, to Jung and Adler, than we are sometimes willing to admit. While American realism "suffers on account of its memories" of Freud, it basks in the humanism of Adler and Jung.

This survey proposes to take no account of the literature on abnormal psychology. But pertinent to the section now under consideration, two borderline studies here deserve mention. Terry (84) reports the case histories of one hundred married women, reminding us of Freud's basic assumptions in respect to the normality of abnormal sexuality, as well as the psychodynamic biographical patterns which determine the flowering-out of personality. Hopkins (47) out-Freuds Freud in his ability to rake up doubtful data in support of his thesis that "the attitudes which had their origin in the infantile relationship of father-son are often the true cause of the adult's orientation toward particular social movements."

The Adlerian influence is in a measure still indirect. Two volumes by Wexberg (98) and Künkel (54) remind us of the popularization program that made Freud famous in America. More important in certain respects is the work of Vaughan (90) and of Walsh (94), who supplement each other in their decidedly popular interpretations of superiority and inferiority respectively. Still more important is the work of the Allports (3 and 4), who have supplemented actual experimental work by plausible theoretical considerations methodological in nature. At the same time Bender (9), Lehman (57), Gardner and Pierce (31), Chassell (18), and Hunter and Brunner (49) have contributed much to our knowledge of developmental personality in terms of Adlerian principles, dealing generally with college students.

When we approach personality studies from what might be called the Jungian point of view, several investigations attract attention. Jung's basic types, introverts and extraverts, are no longer considered poetical classifications. While pure types are rare, their modifications, plus and minus, run deep. Furukawa (30) by blood tests found "active" and "passive" temperaments, which correspond to extravert and introvert respectively. Rich (78) believes there is some relationship between one's personality and one's chemical make-up, and McDougall (68) thinks that introversion and extraversion are associated with thyroid secretion. Whitman (100) finds no differences worthy of mention between the sexes on the question of introversion and extraversion. At the same time these dispositions

tend to become settled types at increasing age-levels. Broom (13) finds that introverted students below the average as rated by standard intelligence tests show a tendency to more facile school adjustment than do the extraverted who rate higher by the same intelligence tests. He finds reason, however, to avoid extreme generalization on this point. Whitman (99) proposes a short scale for determining introversion and extraversion, suggesting possible vocational applications, as do Hewlett and Lester (46) and Neymann and Kohlstedt (72). Campbell (16) finds limited test applications for introversion and extraversion among the insane, indicating how fundamental these dispositions are, while Powers (75) suggests the values of the Marston Introversion-Extroversion rating scale in the domain of pedagogy. More general in nature is the work of Gilliland (35), who suggests the outstanding problems of personality. Frank (29) follows Kretschmer's classifications in determining socially sanctioned patterns in terms of capacities to learn. From the theoretical point of view Prince's work (76) deserves prominent mention.

In all these studies we note a tendency to regard personality as the axial center of social psychology, with a decided preference to evaluate social behavior in terms of introversion and extraversion. These dispositions are names for integrated patterns on both the cognitive and autistic levels of mentation. A good brief statement of the definition of these types in the historical setting is set forth by Guilford and Braley (38).

Group Psychology. The next higher pattern integration is the group, which strict behavioristic psychology regards as a "fallacy," and which sociologists of all schools regard as a reality. The battleground is for the most part a matter of definition of terms, and methodological concepts. Much depends upon the attitude of individual students in respect to the structuralization of social psychology as a science. The problem may be formulated as follows: Are the logical methods of the natural (physical) sciences applicable to the social sciences? Very little thought has really been given to this question, although it seems to be vaguely conceived by a number of students, chiefly those possessing a thorough training in psychology.

F. H. Allport (1) takes up the case in favor of the natural science method as opposed to the historical method, and accordingly suggests that such terms as "group" and "institution" have no valid meaning for the social sciences. Lundberg (62) champions Allport's position with remarkable clearness, considering the fact that he would include all the social sciences in the application of what he considers the

"logic of sociology and social research." Lundberg argues that statistical methods promise much more than they have already done; that the prevailing ignorance of social science and an unwarranted worship of pedagogical expediency are the chief reasons for the tardy application of the logic in question.

Over against the position of both Allport and Lundberg, one must place the warning of Haldane (39) who submits that "Psychology must be regarded as a branch of knowledge which deals not with the relatively abstract aspects of experience dealt with by mathematical, physical, and biological sciences, but with the more concrete experience which is that of conscious behavior and the interest and values expressed in it" (p. 139).

As matters stand to-day, this problem of method is suspended in mid-air. When it has finally settled down, the categories of social psychology will undergo a profound readjustment, not only in respect to their relative importance as such, but also in respect to their nominal identities.

Despite the fact that the natural science method is the one and only logical procedure to employ in the explanation of most psychological phenomena, it does not apply, nor can it be applied, with strict rigidity to the entire field of social psychology in the light of the present categories of that science. Depending on the way you look at this problem of method, individuals are just as fallacious as groups. Yet both are realities. The whole problem is discussed by Markey (65) who has shown, I think, that personality is meaningless, if groups are meaningless. This work is unquestionably the most important contribution to the methodological aspects of social psychology during the period under consideration. Pertinent also to this general theme is Lundberg's (63) study of the problems of social research and Bernard's (11) statement of the development of methods in sociology; also Heard's (45) thesis that the evolution of civilization may be regarded as a series of developmental stages culminating in a superconsciousness. In a similar vein Dewey (21) argues that society is developing into a corporateness of huge dimensions and import. Eubank (23) submits that "a social group may be regarded as an entity consisting of two or more persons in active or suspended action."

Passing from the general to the more specific aspects of the psychology of group phenomena, the usual variety of studies is reported. Benedict (10) in a rather popular article shows that symbolism is connection between cultural patterns and the meaning

individuals attach to them, while Broom (14), Anderson (5), Hunt (48), and McClatchy (67) take up the question of "social intelligence," suggesting the possibilities of one day being able to tell more about the capacity of groups to assimilate the essentials of progressive culture.

Under the caption of public opinion Thurstone (88) undertakes to establish a theoretical basis for its distribution. This study should serve the important function of evaluating the work of Townsend (89), Vaught (91), Faris (25), South (81), Paget (73), Lumley (61), Sheffield (80), and Wallace (93), all of whom are concerned with the problems relating to the fluctuation of and training for social intelligence, as a factor in institutional evolution.

Among the earlier social psychologists there was a division of opinion as to the sort of influence the group had on the individual. Le Bon, for example, held that the individual suffered a loss of reasoning ability, when exposed to group influence, while McDougall held that group intelligence was superior to that of the most intelligent member of the group. Two studies revive this old question. Watson (96) finds from the small groups he studied that "most of the factors that make for efficient work as a member of a group, lie outside the range of the things we are doing in education to equip individuals to do tasks by themselves." Farnsworth (27) cannot find a consistent significant group effect.

On the question of "sympathetic induction of the emotions," which shared a place of importance equal to that of the now exploded instinct theory, in differentiating Allport from McDougall (earlier mentioned as the historical forerunners of contemporary psychosocial outlook), some minor studies deserve mention. Two studies, by Landis (56) and by Guilford (37), bear out in the main Allport's contention that the theory of sympathetic induction of the emotions is not a valid explanatory principle for interstimulation and response.

Cultural Psychology. Aggregates are the carriers of two sorts of stimuli for the individuals who compose the aggregates. In one case the individual is stimulated by his human situation as a member of a group of persons; in the other the individual is stimulated by the values which the aggregates entertain in respect to a common culture. There is therefore a psychological distinction (even though it be a loose one) between social psychology and cultural psychology. Stern's (82) discussion of the distinction between the social and the cultural is easily the most pointed study during the past year or two.

What is known variously as "cultural psychology" or "social

anthropology" represents the highest order of pattern formulation. Race and culture are related genetically, but tend to lose this identity in the course of social evolution. Studies by Lowie (59), Wissler (101), Malinowski (64), and Thomas and Znaniecki (86) may be taken as representative foundations for more recent investigations.

Social anthropology or cultural psychology is the most complex of all the social sciences, and not least among its components are the systematized mental attitudes which a cultural group maintains in respect to its round of life. Upon this point no one can say where social psychology may be separated from sociology. At all events psychology is indispensable for an understanding of culture, which is equivalent to wants, values, and needs (7). The problem of method here reaches its highest point of complexity. It will be many years before the natural science advocates will be able to disprove the fact that a given cultural group does not live, at least psychologically, in a world of traits which owe their origin to social and material forces operating over long periods of time. Such traits cannot be disregarded in the same fashion as a physicist may decide to ignore a few million electrons and protons because they befuddle his mathematical language too much. Consequently one trend to-day is to regard cultural groups as the units of cultural (social) psychology. As Kantor (52) well says: "Social psychology is the science of conventional reactions to institutional stimuli, and since these phenomena are inevitably connected with aggregates of individuals, the data of social psychology are *ipso facto* connected with groups" (p. 11).

The literature on cultural anthropology is voluminous and international in scope. British scholars (7) are on the whole more theoretical than are American, since the latter live at a time and in a place where a variety of cultures are rapidly passing forever beyond the reach of first-hand investigation. On the other hand, world politics in its present state encourages a search for the real mental differences between races, nations, and classes.

Faris (26) regards the whole question of race as a verbal battle merely, stressing the import of assimilation and the popular tendency to translate political facts into the language of fictitious racial traits. Garth (32, 33, 34) finds that skin, hair, and eyes are racial but mind is not, and again that intelligence is a human trait of great variability in all races. Davenport (19), Porteus (74), and Kuo (55) suggest the important factor of environmental influences in all that goes for racial intelligence, as does Thompson (87). At the same time Viteles (92) finds that inheritance cannot be disregarded altogether.

Pruette (77) suggests that race prejudice in no way resembles instinct, while Hankins (41, 42) feels that race antipathy is likely to persevere merely because of competitive contacts among races.

With the possible exception of language, religion has been the most important of all the cultural complexes found among people. Janesch (50), Flower (28), Grant (36), and Josey (51) have made important contributions to this phase of social psychology.

The Beards (8), Lowie (60), and Ellwood (22) have contributed systematic treatises on the subject of civilization and culture, showing that human history may be written in terms of the diffusion and transformation of culture.

Recent Textbooks in Social Psychology. The status of any evolving science at any given period is best portrayed in the work of those who attempt to systematize it from time to time for pedagogical purposes. One notices in such ventures the enormous influence of taste, interest, and temperament, as well as the weight of contemporary social problems. As Kantor (52) aptly points out, "The data of social psychology are in a peculiar sense intimately connected with one's self. Owing to this circumstance one tends to inject one's own behavior situation into the facts studied and thereby misinterprets them" (pp. 65-66).

So, too, Kinder (53) deplores the absence of scientific technique in the social sciences, ascribing the situation to the fact that each investigator is a part of the very material he would investigate. One wonders if the time may not come when a thorough sociopsychanalytical purgation may become a prerequisite for all those who would qualify as investigators.

The few textbooks which have appeared during the period under review (1928-1929) adequately illustrate this observation. Wells (97) elaborates into a text his descriptions of individual and social restraint, which seems to him to be the all-important subject-matter of social psychology. "It seems quite within the realm of possibility that civilization may be shattered by, as it were, the uneven expansion and contraction of its constituent materials" (p. v). In a similar but wider scope, Stratton (83) surmises that the all-important pedagogical need is that of "presenting to classes in college and university and to general audiences those aspects of international conduct which can best be understood with the help of social psychology" (p. vii).

In contrast with these more or less cosmopolitan views, we find definite attempts to theorize about local problems of a psychosocial

nature. Harland (43), as might be expected from a student interested in the dissemination of information to and for the working classes, emphasizes the relatively chopfallen theory of class struggle, while Murchison (70) narrowly and confusedly insists that a "description of the mere facts of inequality is all that . . . is necessary for an adequate description of social phenomena" (p. 196). Along with these Mukerjee and Sen-Gupta (71) and Ewer (24) prefer to adhere to conventional categories in their respective elementary texts, while Kantor (52) ably revives and interprets the categories of Lazarus and Steinthal, now nearly one hundred years old. In all these one finds some traces of the "Gestalt" or "configuration" psychology, which as such may be regarded as a systematization of the "pattern" psychology which, in this review, has been represented as the axiological concept in recent study and achievement.

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RECENT STUDIES IN SEX DIFFERENCES

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I. *General.* Since 1927, the field here to be reviewed has opened up at an increasingly rapid rate. A good beginning can be made with Langdon-Davies' popular chapter on the "history of human ideas about sex differences" (79). Five factors are cited as causing the myth of the "female character," and a prophecy of the future of womankind is attempted. Sumner and Keller (124) present a vast amount of carefully gathered and documented data on the social side of sexual relations, especially in primitive man. Briffault (21), a British anthropologist, offers extensive and exhaustive documentation for his thesis that the basis of human culture lies in the primitive sentiments about woman in her reproductive capacity; his three volumes contain approximately 2,300 pages. Bauer (10, 11), a Viennese gynecologist, tries to prove that sex determines all of woman's actions, no matter how trivial, from birth to death, citing many references in his study of the physical, mental, sexual, and erotic aspects of feminine life. Scheuer (113) offers a historical contribution to the psychology of sex. Heilbron (60) believes woman to be inferior, "above all in the central organ, the brain." A good bibliography of titles, from classical literature to modern experiment, is found in Weith-Knudson's popular book (128). The feministic note is represented in such studies as that by Kennedy (76), using a Japanese setting.

Turning to bibliographical studies, it will be unnecessary to more than mention the following: Allen (2), Ellis (44), Lincoln (86), Louttit (88, 89), Scheuer (114, 115), and Woody (135); these give the careful student the key to the better material. In addition, Goodenough's (54) statement of a program for future research, together with a short summary of some studies in this field, is important and stimulating. Current work on sex differentiation and sex determination in animals is reported in such periodicals as the *J. of Exper. Zoölogy* and the *Biol. Bull.* for 1928 and 1929 especially.

II. *Physical Differences.* Anatomical studies may be passed over rapidly in favor of studies of behavior differences. Over one thou-

sand female students at a state university have been measured by Jackson (74), emphasizing body-build and vital capacity. Kjer-rulf (77) contributes height and weight measures on 1,244 Swedish children between seven and fourteen years of age. Heart action (87), bone growth (112), myopia (119), and growth of head-form (133, 134) have all been subjects for careful study. They are not here discussed, but the references given briefly abstract the source material.

We shall find more of interest in the studies of the *functional* differences. An insight into the problems involved is to be found in the Russian Bielow's (12) book; in such physiological studies as that of Boyden (17) on the gall-bladder; in the endocrine studies of which that of Herschan (62) is representative, claiming to have isolated the factors responsible for women with masculine characteristics. Butenandt (27) is said to have been the first to isolate the sex hormone in its pure form; he calls it "Progynon," and describes it as a stearate.

The *animal* work is important. Allen (3) cites sex differences in monkeys, reviewing the experimental literature also. A more extensive study of the "genetics of sexuality in animals" is offered by Crew (36). Nisson (98) lists the work that has been done on the white rat, using operative techniques; Riddle and Reinhart (110) approach the problem from its more chemical aspect. Hitchcock (65) finds the female white rat more active than the male.

Studies of the effects of *menstruation* upon the behavior and status of woman have been sharply divided into the experimental and the psychoanalytic. A plea for a more careful study of menstrual concomitants (6) seems to overlook the work already done by Clow (31), Conklin, Byrom, and Knips (33), etc., who stress the effects of suggestion and expectancy. On the other hand, the study of the so-called "menstruation psychosis" by Prengowski (107) and by Danby and Sykes (40), among others, leaves the matter unsettled so far as experimental evidence is concerned. The careful study of puberty precox by Gesell (53), and of the cycle in general by Sowton, Myers, and Bedale (121), give more assuring answers to the questions raised, and leave one with the conclusion that menstruation, in the average woman, *need* not incapacitate her for industry or professional pursuits. The fact that it frequently *does* is minor to this generalization, which is supported by experimental research.

The psychoanalytic method is seen in studies by Daly (38, 39), who assumes a phylogenetic factor in feminine inferiority, based on the menstruation tabus. Hirschmann-Wertheimer (64) finds two

types of reaction to menstruation. In these studies, the analytic concepts have been freely used, and must be evaluated in the light of one's attitude toward those concepts.

III. *Mental Differences*. Bühler (25) discusses sex differences, considering different phases of maturity as the decisive factor. Chesser (29) offers a book to help woman understand "her inter-related physical and spiritual forces." The recent revision of Ellis' *Man and Woman* (43) needs no further comment. In addition, the stimulating work of Kelley (75), attempting to determine quantitatively any mental traits that may exist between the sexes, must be mentioned.

1. The first of the special psychological problems, the changing *moral standards*, is examined by Emery (45). Many articles discuss the implications of moral evaluation and character, as in Valentine (125) on judgments of character, and Banning (8), reflecting "modern" ideas.

2. *Interests and drives* are favorite topics for study. An exhaustive study of the entire range of interests and activities of women, compiled by Clarke, etc. (30), is enlightening. More experimental in tone is the study of color preferences of white and negro children by Hurlock (69), and of Japanese adults by Mizuguchi and Aoki (96). Imada (73) found marked sex differences, but not in agreement with either of the above. Age and race factors in these studies confuse the issue, however. Lehman and Witty (81, 82) have shown girls to demonstrate esthetic appreciation more clearly, and also to show a greater fondness for, and time spent at, reading for pleasure. In studying the data of an art test given to a thousand unselected grammar-school children, Lewerenz (85) found that the girls excelled in originality and color recognition, but that the boys excelled in art vocabulary, in visual memory of proportion, observation, etc.

3. The great mass of studies relating to *cognitive functions* may be divided into two groups: tests of intelligence, grades, etc., and tests of learning and memory, primarily.

Paterson and Langlie (101), after pointing out that much of the past literature has shown boys to excel girls on intelligence scores, also assert that new techniques of examination—more objective in nature—reduce the discrepancies between intelligence scores and grades, in the latter of which girls excel boys. The thesis is put forth that these scholarship differences are pseudo-differences. Crawford (35) says that girls study more, attend more regularly,

and do better in all save mathematics. Frailey and Crain (51) declare that girls also prepare their lessons better. Several studies, however, present conflicting data. Boys surpass girls on the intelligence tests given by Paterson and Langlie (101), Whipple (130), and by Schultz (117). On the other hand, Goodenough (55) finds that boys, on the average, are poorer on a first Binet test, but that the girls lose ground on a retest. Lentz (83) found that only 42 per cent of the girls tested surpassed the median for boys, while 58 per cent surpassed the median for boys in grades. Whipple (129, 130) cites the results of two testings, in each of which the girls outdid the boys, in all deciles in the one case, and at all ages between eight and thirteen years in the other. But add to the many possible interpretations of these conflicting data the fact that many find no significant sex differences, and the problem is still an open one. Crawford (35) and Davis (41) indicate this position.

There is much more agreement as to achievement in the specific subjects of study. Boys are generally conceded a superiority in things mathematical and concrete; girls in language work, memory, and abstract reasoning. Yet the case is not at all clear. As far geometry, Webb (127) represents the usual finding that boys surpass girls, and especially at the lower MA levels. Perry (104) disagrees, holding that girls attain higher achievement because they are able to jump some steps in reaching the conclusion, which seems to be beyond the average boy tested. In adding, Pauli (102) finds that boys always excell in the original performance, but that girls improve more to cut down this initial advantage. Porter (106) shows that girls are more upset by emotional factors, due to accumulative errors of which they are said to be more aware.

Motor learning has hitherto shown large sex differences (as in the Young slot maze, *e.g.*). McGinnis (94) agrees that boys from four to eight years of age are markedly superior to the "man-and-show" maze, also that practice again favors the girls. This agrees with earlier studies which have found no sex differences in motor coördination, and with Goodenough and Brian (56) who find girls gain more by practice in target shooting. McGeoch (93) finds no large differences, in any of the literature. Husband and Miles (71) find women sort cards faster, both for color and form. Ilyinsky (72) gets a slow and unstable associative reflex in males, and a faster and more persistent one in females. Neubauer (97) suggests that boys are a year ahead of girls in making mechanical toys that function

well, and in adding the technical details to their drawings. Interest factors are obviously present here.

In tests of immediate *memory*, Fischler and Ullert (47) find no great sex differences, using five tests on 450 children and 80 adults. Fisher (48) finds that boys excel at substitute learning at nine, ten, fourteen, and fifteen years; girls at twelve, thirteen, and sixteen years. The improvement curves are different also. Foster (50) finds boys consistently better at verbal memory for stories; this was an unexpected result, inasmuch as the girls had done better on the intelligence test. In history retention, Bassett (9) finds boys slightly better on concrete facts and principles, while girls reflect their greater interest in home life and living conditions. Burt and Crockett (26) find no significant sex differences in recall or recognition of billboards.

Finally, the psychology of testimony, as McGeoch (92) reports, shows that girls are slightly better on picture reports, and much better on event reports. Bird (14) shows the effects of suggestion, as the most important fact in a review of the literature. Miles and Terman (95) incline to regarding them as due to social conditioning. Seward (118) finds no reliable sex differences in speed or relative variability of recognition times, although women as a group rated their self-confidence higher than did the men.

Studies of *ideation* have been made by Miles and Terman (95), who find women more introverted and hence more inclined to the evaluating type of associative response. Wylie (136) reports no sex differences in ideation, and Pyle (109) finds that they diminish with age, especially as compared with motor skills. Griffiths (57) finds very small, though consistent, sex differences in imagery—women better in auditory, and men better in visual; a finding which agrees with much of the earlier data. Eidetic imagery studies seem to agree that females show a higher amount of eidetic imagery than males. Studies by Zeman (137) and by Fischer and Hirschberg (46) are examples of such findings.

4. Greater *variability* has been claimed for each sex at different times. Cf. Allen (2). Frailey and Crain (51) and Commings (32) find boys more variable. The former attribute this to the greater freedom of conduct of boys. However, the matter may best be left with reference to Winsor's survey of the literature (132); he concludes that girls are as variable as boys whenever a large enough sample of a given age has been tested.

5. Women are consistently reported to be more *suggestible* and

emotionally unstable. Heron (61) shows women to be more liable to illusory warmth in his experiments. Bridges (18) claims to find greater emotional instability in college women than in college men, tested by the Woodworth Personal Data Sheet. Lehman and Witty (80) find women more credulous regarding fortune-telling. Levy (84) reports the greatest proportion of resistant behavior in young children under four years of age to come at thirty months in males and eighteen months in females.

The school side of the problem is found in Bridgman's (20) analysis of mental defectives coming to a public clinic. In almost 4,000 cases there were slightly more male idiots and female imbeciles; but many more female morons and more dull and borderline males. It is suggested that the Binet scale may exaggerate sex differences, due to social elements. The incidence of truancy shows more boys at thirteen years and more girls at fifteen years of age (131).

The psychiatric angle to this problem is outlined by Pollock (105), who suggests that there are characteristic age and sex groups for each psychosis, with wide interrelations, however. Psychoanalytic studies by Stadelmann (122), Schilder (116), and Horney (67), and another group relating more to the early formation of personality, include studies by Aikins (1) on the masculine protest, Ceni (28) on the maternal impulse, and Freud (52) on the "castration complex."

6. The more orthodox studies of *personality* differences are those by Allport (4), and by Huntington (68). The latter lists natural selection through migration, geographic conditions, and the overstimulation of some social areas, as the causes of the "uniqueness" of the modern American woman. The extraversion-introversion dichotomy is reported upon by Broom (22), and by Heidbreder (59). The former found no appreciable sex differences; the latter, however, lists certain differences that appear independently of introversion-extraversion.

7. Only one recent serious study of sex differences in *handwriting* is at hand. Broom, Thompson, and Bouton (23) report that untrained judges were able to determine correctly the sex of the writers about two times out of three.

8. *Industry* to-day presents an everchanging front to the woman employee. The Woman's Bureau literature, U. S. Department of Labor (24), shows the mass of information to be had. The chief of the bureau (5) has summarized the program, elsewhere. In contrast with this type of work is the platform of absolute equality demanded by the "Open Door International," founded in Berlin

in 1929 by feminists who were scornful of special legislation for women. However, the majority of trade union and Socialist women are opposed to this new extremist doctrine (37). Much of this literature is in the opinion stage. Symposia (63) have been published, questionnaire data from Y.W.C.A. working girls compiled (99), and the pros and cons argued (42). Reasons why women want to work have been suggested by Groves (58). A more direct attack on this problem is Hogg's analysis of occupational interests of women (66), together with a suggestive study by Bridges (19) on occupational interests of three-year-old children, as revealed by the Montessori materials. Slight sex differences are found agreeing with traditional opinion.

When women do enter business or industry, Bills (13) finds the sexes differ only slightly in length of service. A preliminary study of woman taxi drivers by Viteles and Gardner favored the women. In a later study (126), other variables were controlled, and men were found superior only in training. Women ran up more, but less serious, accidents than did the men.

9. Closely allied to changing attitudes toward women in business and industry, are the changes in the *social life* of the sexes. Banning's (8) article summarizes the popular statements of one phase. Flügel (49) gives six reasons why male "homosociality" is the more highly developed, at the same time pointing out that differences are diminishing in favor of a more "heterosocial trend." Attitudes toward other races (15) reveal some interesting sex differences, pointing out that mistakes in judgment are often more costly to women. Pan (100) and Pedersen (103) show attitudes toward the institution of marriage, and ideals of the proper mate for a woman. In a recent book, Hurlock (70) sums up many facts regarding display, and the differences in masculine and feminine fashions, as they effect social living.

10. Finally, brief mention may be made of the various ideas regarding the *logic* and *intuition* of women. Macy (90) summarizes them popularity. Rodier (111) finds that any such differences are superficial, but that the biological basis is apparent: man's psychology is of the brain; woman's is intuitional and superior to male logic, which is often baffled by the maze of facts to be sorted. In discrimination tests, Kubo (78) cites small differences. Women do better in estimating how much symbol substitution they can do in a fixed time. No sex differences in accuracy were reported.

IV. *Conclusion.* In summing up, there seems no good ground

for changing the three conclusions drawn at the end of a similar study in 1927 (2). They are, therefore, here repeated:

1. Few, if any, of the so-called "sex differences" are due solely to sex. Individual differences usually are greater than differences determined on the basis of sex.

2. The social training of the two sexes is, and always has been, different, producing differential selective forces, interests, standards, etc.

3. A number of variables either cannot be, or are not, controlled, making conclusions uncertain. Among other improvements, a more careful definition of terms is needed.

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SPECIAL REVIEWS

JUNE E. DOWNEY. *Creative Imagination*. N. Y.: Harcourt, Brace, 1929. Pp. viii+228.

A well-known writer on esthetics recently remarked that psychology will never illuminate the field of art so long as it confines itself to triangles, color preferences, and disembodied rhythms. Professor Downey, apparently recognizing the same inadequacy in the traditional psychological method, endeavors in *Creative Imagination* to find more fruitful approaches to the complex problems of esthetics. Not that she scorns the standard experiments—rather she makes profuse reference to them—but she does insist “not only upon the evanescence of imaginal content, its extraordinary complexity, and the possibility of its function in fragmentary and syncopated forms, but also upon the fact that a description in static terms does violence to the dynamic, fugitive onrush of thought, its quicksilver-like darting from one point of stress to another” (113).

The book is composed of twenty-nine short chapters. Written over a period of several years and sometimes published as independent studies, these chapters suffer somewhat from discontinuity. Even their logical ordering in book form fails to forge any systematic bond. But the author does not regard her contributions as providing a single consistent esthetic theory. Her interest is expressly one of description (12). In the course of her rich treatment of the intricacies of creativity she shows herself to be completely at home in the field of literature, willing to experiment and to test hypotheses, but definitely unwilling to sacrifice her comprehension of the total field for any spurious or partial theory.

Several of the chapters are analytical in nature, especially those which are concerned with imagery. While admitting individual variation in imagery, as in everything esthetic, the author speaks perhaps a little too frequently of types. Regarding eidetic imagery she writes, “The point of contact between my conjectures and those of the Marburg School is that both recognize certain features of visualization as symptomatic of psychophysical trends; both describe a rigid image in contrast to a flexible one; both find the latter characterizing the more imaginative individual. The conjectural physiological explanations in the two cases are different. Mine is quite

frankly obscure; the Marburg hypothesis is backed by some evidence in the field of internal medicine"(41).

Other chapters show a preference for the configurational method. "A poem is, in many ways, the finest possible example of a psychic pattern in which the ensemble determines each detail"(126). The "Introspective Snap-Shot" given by a creative writer (117-121) illustrates well the "total field" within which the configurationist must seek for the dynamical properties of inventiveness. Miss Downey regards the outstanding problem of creativity "why some persons' cogitations (conscious or unconscious) issue in *original* patterns, others in stereotyped or conventional ones"(160). In solving this problem the Gestalt conception of the unconscious seems to her to hold more promise than the psychoanalytic, which though it makes fruitful suggestions regarding motive, fails completely to account for talent or style.

A book dealing almost exclusively with the psychology of poetry should perhaps give more attention to the metaphorical consciousness than does the author's brief chapter on this subject. Much that she includes elsewhere under synesthesia, style, expressionism, and cartooning, might be brought under a more spacious and consistent treatment of the metaphorical process. Instead she is somewhat preoccupied with the imagery experienced by observers in response to the main and accessory parts of the figures of speech, and with the common background of mood from which the two items emerge and through which they are joined. The basic formula for poetry would seem to be the statement of a dominant theme (an object to be described or a mood to be interpreted), with elaboration by the aid of relevant but subordinate percepts. From this point of view the keystone in the edifice of poetry is the metaphor, and the metaphor is essentially a problem in the psychology of thought. With this central fact in mind, one can pass to the affective aspects of poetry without losing sight of its fundamentally conceptual nature. The humor resulting from the incongruous metaphor, the joy at the apt metaphor, the variational factor in the appreciation of metaphors, and many other aspects of the total problem could be neatly ordered around this basic conception.

The strength of the book lies in its adroit handling of special topics. Outstanding in brilliance are the chapters on "The Word-In-Itself," "The Poetry of Colour," and the two sections on psychological distance. One leaves this psychoesthetic panorama feeling that the author comprehends the complexity of her subject, and for that

reason has been talking about true art and genuine creation. With work of such horizon and such understanding available there should be less complaint from artists concerning the sterility and "impudence" of the psychological method.

GORDON W. ALLPORT

Dartmouth College

EDMUND S. CONKLIN. *The Psychology of Religious Adjustment*. N. Y.: Macmillan, 1929. Pp. xiv+340.

This book will serve as a convenient manual, summarizing as it does the main features of the psychology of religion, and citing the more important literature, including the latest studies. The psychological standpoint of the author may be characterized as functional, with a leaning toward a mild form of behaviorism. He has familiarized himself with all the better known religious communions in this country at the present time, and speaks with a knowledge derived not only from books but from first hand observation.

The psychological interpretations are most satisfactory with reference to contemporary American religious experience; attempts at universal statements are less convincing. For instance, it is asserted in italics that "*religion may be defined as designating that behavior and those behavior products which are associated with a belief toward some concept of a god or gods*" (p. 47). This is a good definition of religion as it exists in this country to-day, except that it leaves out Humanism. On the other hand, the definition can hardly be made to include the religions of some primitive peoples (*e.g.*, the aborigines of Australia), and Hinayana Buddhism; it at least puts the emphasis in the wrong place for most other forms of Buddhism, as well as for Confucianism.

A good analysis is made of the religious sentiment, which is distinguished from religious attitudes. The sentiment, which is acquired and does not have precisely the same constitution in all individuals, comes to include emotions of awe, wonder, submission, fear, love, and reverence. There is no specifically religious instinct. From a psychological point of view conversion and sanctification, both of which may occur repeatedly, must be regarded as forms of religious experience that are more intensified than worship and prayer, but are not unique experiences of a different order. Mystical experiences furnish a complex problem. They vary enormously. Among the factors that must be taken into account in their interpretation are motives, attitudes established, retraction of the field of consciousness,

free development of certain emotions, and the effect of introspective periods upon extravertive adjustments (p. 165).

Particularly interesting is the discussion of the appeals made by American churches. These vary in emphasis on intellectual, ceremonial, and ethical interests; they all arouse emotion in some form, and would not be religious if they did not (pp. 84 ff). A comparison of the orders of worship in non-liturgical, Episcopalian and Roman Catholic churches discloses similar efforts to arouse a definite sequence of thoughts and emotions: first, awe (including inferiority, wonder and fear), followed by tender emotion (induced by a prayer of confession and assurance of forgiveness). Jewish worship evokes emotions generally similar, but not in so precise a serial order. It appears impossible for any particular type of service to appeal equally to persons who differ so widely in their religious emotions and sentiments as Americans do to-day. This partly explains why religious appeals are often weak and ineffective. Suggestions are offered for the improvement of religious education in the effort to remedy this situation.

WILLIAM KELLEY WRIGHT

Dartmouth College

BERNARD C. EWER. *Social Psychology*. N. Y.: Macmillan, 1929. Pp. 436.

Fully aware of the breadth and diversity of the subject he is to attack, Professor Ewer defines the present task of social psychology and the purpose of his book as "that of synthesis and organization"(vi). This book's "distinctive features are its explicit statement of the nature of higher and lower levels of social process, and its emphasis upon thought as a supremely important social function"(vi).

In the first part of the book, "Fundamental Activities," we find an attempt to work out some method to study "the traits of mind which underlie social life"(6). The author says that he is trying to weave the ideas of McDougall and F. H. Allport into "a unitary synthesis" of his own. Hence the starting point is the individual and the ultimate purpose is to explain that social consciousness or "feeling-attitude"(21) which the individual acquires in social relationships. The explanation proceeds on the basis of the four levels of instinct, learning, imitation, and reflective thought.

After pointing out F. H. Allport's criticisms of McDougall's definition of instinct, the author tries to steer a middle course by

interpreting the term instinct as "a spontaneous interest in a certain kind of object, with more or less definite response to its presence, and the development of emotion in relation to it" (57). Further compromise is seen in his list of the most noteworthy instincts which are "gregariousness, domination and submission, pugnacity, sex, and parental care" (60). If the author has avoided landing himself in an antinomy, he has done so, perhaps, at the sacrifice of any basic, rigid explanation. There seems to be throughout a tendency toward over-simplification in accounting for the dynamism and the mechanism of behavior. His general consideration of learning is open to the criticisms of *Gestalttheorie*.

In Part II there is a description of those factors which go to make up "Individuality." Here the author finds himself on much surer ground and his competent observation continues into Part III on the "Social Order." This is a discussion of class, crowd, organization, custom and fashion, morals and manners, and of social control, conflict, and progress. No new interpretations are offered but the various phenomena are clearly and ably handled. Part IV contains "Appendices on Social Behavior Systems" such as speech, play, religion, patriotism, etc.

Negative criticism might point to the rather hasty explanation of so important a social factor as the acquisition of language, the use of terms in the beginning which are not clearly defined (the book is meant for elementary use), and the lack of bibliographies. On the positive side we find that the author does not overlook the important part that integration and the tendency towards unity play in behavior. In general the book is weak in basic explanation but rich in description of social behavior.

HADLEY CANTRIL

Harvard University

BOOKS RECEIVED

EDWARD B. TITCHENER, *Systematic Psychology: Prolegomena*. N. Y.: Macmillan, 1929. Pp. xi+278.

C. JUDSON HERRICK, *The Thinking Machine*. Chicago: Univ. of Chicago Press, 1929. Pp. xii+374.

ALEXANDER HERZBERG, *The Psychology of Philosophers*. (Trans. by E. B. F. Wareing.) N. Y.: Harcourt, Brace, 1929. Pp. x+228.

FRED A. MOSS, *Applications of Psychology*. Boston: Houghton Mifflin, 1929. Pp. x+477.

RAYMOND H. WHEELER, *The Science of Psychology*. N. Y.: Crowell, 1929. Pp. xvii+556.

W. B. PILLSBURY, *The History of Psychology*. N. Y.: Norton, 1929. Pp. 326.

S. W. and J. T. PIERCE, *The Layman Looks at Doctors*. N. Y.: Harcourt, Brace, 1929. Pp. 251.

JOSEPH PETERSON and LYLE H. LANIER, *Studies in the Comparative Abilities of Whites and Negroes*. Mental Measurement Monographs, 1929, No. 5. Pp. vi+156.

DOROTHY SPEER, *An Experimental Evaluation of Seven Composition Scales*. Johns Hopkins Univ. Stud. in Educ., 1929, No. 14. Pp. x+85.

L. L. THURSTONE and E. J. CHAVE, *The Measurement of Attitude*. Chicago: Univ. of Chicago Press, 1929. Pp. xii+96.

EDOUARD LE ROY, *La Pensée intuitive*. Paris: Boivan. Pp. vii+204.

ROBERT S. WOODWORTH, *Psychology* (Revised Edition). N. Y.: Holt, 1929. Pp. xiii+590.

JAMES H. DUNHAM, *Principles of Ethics*. N. Y.: Prentice-Hall, 1929. Pp. xiii+570.

ALBERT P. WEISS, *A Theoretical Basis of Human Behavior*. Columbia: Adams, 1929. Pp. xvii+479.

ROBERT SAUDEK, *Experiments with Handwriting*. N. Y.: Morrow, 1929. Pp. 394.

J. E. W. WALLIN, *A Statistical Study of the Individual Tests in Ages VIII and IX in the Stanford-Binet Scale*. Mental Measurement Mono., 1929, No. 6. Pp. vii+58.

JACQUES CHEVALIER, *L'Habitude*. Essai de metaphysique scientifique. Paris: Boivin, 1929. Pp. xviii+256.

V. E. FISHER, *An Introduction to Abnormal Psychology*. N. Y.: Macmillan, 1929. Pp. x+512.

BERNARD C. EWER, *Social Psychology*. N. Y.: Macmillan, 1929. Pp. ix+436.

Western Australia, Department of Public Health. State Psychological Clinic. *Annual Report for the Year June, 1927-1928*. Perth: Simpson, 1928. Pp. 24.

EDMUND S. CONKLIN, *The Psychology of Religious Adjustment*. N. Y.: Macmillan, 1929. Pp. xiv+340.

HERBERT S. CONRAD and HAROLD E. JONES, *Psychological Studies of Motion Pictures*. III. Fidelity of Report as a Measure of Adult Intelligence. IV. The Technique of Mental-Test Surveys among Adults. Univ. of Calif. Pub. in Psychol., 1929, 3, 246-284.

MERLE H. ELLIOTT, *The Effect of Appropriateness of Reward and of Complex Incentives on Maze Performance*. Univ. of Calif. Pub. in Psychol., 1929, 4, 91-98.

E. C. TOLMAN, R. C. TRYON and L. A. JEFFRESS, *A Self-Recording Maze with an Automatic Delivery Table*. Univ. of Calif. Pub. in Psychol., 1929, 4, 99-112.

R. RUGGLES GATES, *Heredity in Man*. N. Y.: Macmillan, 1929. Pp. xiii+385.

W. CARSON RYAN, JR., *The Literature of American School and College Athletics*. N. Y.: Carnegie Foundation Bull. 24, 1929. Pp. xlv+305.

SAM R. LAYCOCK, *Adaptability to New Situations*. Baltimore: Warwick & York, 1929. Pp. xi+170.

LOUISE A. NELSON, *Variations in Development and Motor Control in Goiterous and Non-goiterous Adolescent Girls*. Baltimore: Warwick & York, 1929. Pp. xii+193.

NEAL BILLINGS, *A Determination of Generalizations Basic to the Social Studies Curriculum*. Baltimore: Warwick & York, 1929. Pp. xi+289.

A. A. ROBACK, *Jewish Influence in Modern Thought*. Cambridge: Sci-Art, 1929. Pp. 506.

FRANCIS AVELING, *The Psychological Approach to Reality*. London: Univ. of London Press, 1929. Pp. xi+251.

NOTES AND NEWS

DR. CHRISTINE LADD-FRANKLIN, lecturer in psychology and logic at Columbia University and known for her work in color vision and symbolic logic, died on March 5, in her eighty-third year.

PROFESSOR EUGENIO RIGNANO, professor of theoretical philosophy at the University of Milan and editor of *Scientia*, died at Milan on February 9, 1930.

DR. G. HEYMANS, professor of psychology at the University of Groningen, died on the 18th of February in his seventy-second year.

PROFESSOR KARL M. DALLENBACH of Cornell University, will teach in the summer session at the University of Oregon. During the session of 1930-1931 Professor Dallenbach will be Visiting Professor of Psychology at Columbia University, New York City, during the absence of Professor R. S. Woodworth on sabbatical leave of absence.

PROFESSOR K. S. LASHLEY of the University of Chicago was elected to membership in the National Academy of Sciences at the recent meeting held April 30, 1930.

DR. CARNEY LANDIS, Associate Professor of Psychology at Wesleyan University, has resigned his post to accept the position of Research Associate in Psychology at the New York State Psychiatric Institute and Hospital, Columbia Medical Center, New York City, effective September 1, 1930.

At the annual meeting of the Ohio College Association, Horace B. English of Antioch College was elected President of the Section of Psychology and Martin Remp of the College of Wooster was elected Secretary-Treasurer.

PROFESSOR JAMES P. PORTER of Ohio University was recently elected Vice-President of the Ohio Academy of Sciences and Chairman of the Section of Psychology.

EDITORIAL NOTE

It is with great regret that the Editorial Board of the Publications of the American Psychological Association announces the resignation of Professor Madison Bentley as Editor of the *Journal of Experimental Psychology*. Professor Bentley has been associated with this group of journals for many years, formerly as editor of the *Psychological Index* and more recently as editor of the *Journal of Experimental Psychology*. Beginning with the June issue, Professor Samuel W. Fernberger of the University of Pennsylvania, since 1925 editor of the PSYCHOLOGICAL BULLETIN, will assume the editorship of the *Journal of Experimental Psychology*. The Editorial Board is pleased to announce the appointment of Professor Edward S. Robinson of Yale University as editor of the PSYCHOLOGICAL BULLETIN, to take effect with the June issue.

THE PSYCHOLOGICAL BULLETIN

EDUCATIONAL PSYCHOLOGY

BY V. A. C. HENMON

University of Wisconsin

The accompanying bibliography from April, 1929, to April, 1930, includes references of interest to the educational psychologist, exclusive of intelligence tests, educational tests, statistical methods, and tests of personality or character.

1. *General Treatises.*

Texts for use in college or normal school classes in educational psychology covering in the main the usual topics in such courses are provided by Pintner (131), by Collings and Wilson (33), and by Gast and Skinner (60). For use in classes in child study are the revised edition of Kirkpatrick's well known text (95), the new texts by Strang (159), and by Swift (160). Here also would probably belong the study of the child's conception of the world by Piaget (130) and the treatment of the process of human behavior by Sherman and Sherman (148). The psychology of adolescence is comprehensively treated by Brooks (23), and certain aspects of adolescence in the more restricted presentations by Schwab and Veeder (145) and by Wheeler (179). The twelfth edition of Spranger's well known book (152) should be mentioned.

Books treating more specifically of the application of psychological principles to methods of teaching include Burton (25) on the nature and direction of learning, Laton (100) on applications to health education, Mossman (118) on principles of teaching and learning in the elementary school, Palmer (127) on directed learning, and Thorndike and Gates (165) on elementary principles of education. Two additions to the books on the psychology of elementary school subjects are by Garrison and Garrison (58) and by Schmidt (143).

The practical application of the principles of individual psychology in school procedure are set forth by its chief exponent, Adler (2), and for the applications of psychoanalysis in education there is the third edition of Pfister (129). On psychological theory in relation to education, attention should be called to the analysis of conflicting theories of learning by Bode (19). Laycock (101) accepts the basic laws of Spearman on the nature of ability and suggests the rewriting of the psychology of school subjects. Zeman (190) discusses the practical significance of eidetic imagery and eidetic psychology.

The educational psychologist will find of special interest and value certain chapters in the *Foundations of Experimental Psychology*, and they are separately listed in this bibliography. They are the sections on nervous mechanisms in learning by Lashley (99), on experimental studies of learning by Hunter (89), on general ability by Pintner (132), on special abilities and their measurement by Freeman (54), and on the individual in infancy by Gesell (65). He will also find certain chapters worth consulting in the volume on applications of psychology by Moss (117).

Among foreign systematic presentations of educational psychology are the volumes by Döring (46), by Kroh (98), by Schulz (144), and by Simoneit (149). Raup (138) gives an interesting characterization of psychology and education in America.

2. *General Psychology of Learning.*

Those interested in the formulation of the laws of learning in terms of the conditioned reflex should consult the discussions by Hull (88) and Williams (181). Carroll (29) and Stephens (155) take up again the perennial problem of the law of effect. A comparison of human adults and animals in maze learning is reported by Husband (91) and of blind as compared with sighted children by Knotts and Miles (96). Manual skills are studied by Gemelli (62) and Pear (128).

A test of Ebbinghaus' results on the proportional effect of over-learning with negative results is given by Cuff (39). The comparative retention values of maze habits and nonsense syllables by McGeoch and Melton (105) shows surprisingly enough greater retention of syllables. Cheng (32) and Harden (78) studied retro-active effect in relation to degree of similarity. The moot problems of whole and part learning and the relation of initial performance

to final performance are attacked by Crafts (36) and by Hartman (80), respectively.

The problem of motivation comes in for various studies. Crawford (37) studies the general problem of incentives to study on a basis of student opinion. In experimental studies Bills and Brown (13) find that the greater the amount of work for which the individual is set, the higher the initial level of efficiency, Deputy (44) that the effect of frequent knowledge of success depends on student attitude and that there is not the significant improvement shown in laboratory experiments, Jones (94) that words with high emotional values are better remembered, and Jersild (92) that under certain conditions examination is an aid in learning. Maller (108) reports an experimental study of cooperation and competition. Sanderson (142) reviews the literature on the effect of the mind's set in motor learning and gives additional experimental evidence.

Elusive problems are those attitudes and interests and other measurement. Thurstone (167) discusses a new method for measuring attitudes to take the place of correlational methods. Duffy's monograph (49) on tensions and emotional factors in reaction, the study by Freeman (55) on the influence of attitude on structure in learning, the measurement of interest differences between students of agriculture and engineering by Remmers (139), and the further studies of children's interest in collecting by Witty and Lehmann (184) are contributions in this field.

Factors conditioning mental work are investigated in the studies by Pollock (133), by Stainer (154), and by Weber (174). The effect of the summer vacation on achievement of pupils is shown by Nelson (122) and by Morgan (116).

3. Psychology of School Subjects.

1. *Reading.* Further eyemovement studies by Tinker and Pater-son (169) on the effect of length of lines with size of type and constant show that an 80 mm. line gives the fastest reading. Miles and Segel (111) report a clinical method of studying eyemovements in the rating of reading ability not involving photographic registration, and Miles and Bell (110) study eyemovements in relation to study habits. Hovde (87) finds that in a study of effects of size of type, leading, and context, that reading rate is determined almost entirely by context.

Analyses of processes involved in reading are given systematically

by Book (20, 21), experimentally by Pyle (135) and Dawson (42). German investigations of reading of note are by Thorner (166), on the reading of nonsense material and by Heimann and Thorner (82) on the reading of meaningful material.

Reading interests and habits of adults are surveyed in the volume by Gray and Munroe (73) and of various adult groups in the study by Grace (72). Reading interests of 487 high school girls are reported by Elder and Carpenter (50).

Two interesting studies of non-readers and remedial procedures are given by Dearborn (43) and by Stone (158). Pressey and Pressey (134) and Carroll and Jacobs (30) show striking gains in experiments on training college freshmen to read. Berry and Stoddard (12) report an experiment with 172 lispers showing marked improvement after eight months training.

2. *Handwriting.* Conrad and Offerman (35) report a motion picture study of how manuscript writing can be acquired, Gates and Brown (61) find print script to be learned a little more rapidly than cursive writing in the first half year, while cursive writing seems clearly a more rapid form for grades four to six, Guiler (75) shows methods of securing improvement in handwriting, and Newland (123) analyzes the illegibilities of arabic numerals.

3. *Spelling.* Horn (86) finds no hard spots in words common to pupils and infers that the learning of each word is an individual problem. Abernethy (1) analyzes the eyemovements of good and poor spellers. Book (22) shows how special disability in spelling of a twelve-year-old was diagnosed and corrected. Guiler (76) compares oral recall, written recall and multiple choice in testing spelling. Stone (157) criticizes interestingly spelling texts.

4. *English.* Symonds and Lee (161, 162) continue studies in learning English expression with investigations of capitalization and the growth of vocabulary in written composition. Leonard (102) finds large gains in four exercises in proofreading, error correction, and dictation on ability in composition with reference to capitalization and punctuation. Bennett (110) finds brief drill periods better than discussion periods of reasons for using certain expressions in developing language ability.

5. *Arithmetic.* As usual the number of experimental studies in arithmetic is large. Buswell (26) summarizes recent investigations. Brownell (24) reports large gains from six weeks individual instruction after diagnosis, Chase (31) shows diagnosis and treatment of common difficulties, Guiler (74) gives methods of correcting errors

in four fundamental operations, Mitchell (112) finds distinct difference in favor of specific over general problems, Morgan (115) similarly specific diagnostic and remedial exercises better than general material, Renwick (140) provides special methods for securing accuracy in concepts in mensuration, Stainer (154) determined the time of day when most work in arithmetical computation was done, and Wheat (178) found no significant advantage for imaginative over conventional types of problems.

4. Controlled Experiments on Methods

Archer (5) in a study of spelling investigated the transfer effect of the study of words upon selected derived forms and on words similarly constructed but not derived. Melby and Lien (109) made a study of two methods of teaching, employing not one control and one experimental group but several. Russell and Long (141) in comparing two methods of instruction in mathematics, find individual instruction superior. Simpson (150) found distinct gain from training in answering questions, evaluating, outlining, and summarizing on ability to read and study history. Washburne (173) likewise found inclusion of questions resulted in a significant difference in recall and understanding of historical material. Willard (180) reports the Dalton plan superior to daily recitation procedure in learning history.

5. How to Study

Muse (120) provides a manual for use of college students on study habits. Woodring and Flemming (186) issue a volume on study for high school pupils. Althaus and Gilliland (13) in a controlled experiment find that instruction in how to study does not improve school work.

6. Visual Education

Wood and Freeman (185) give the results of their studies carried on for the Eastman Company. Knowlton and Tilton (97) set forth the results of experiments with the Yale chronicles in history teaching. Lewerenz (103) shows the results of a visual education lesson taught with the aid of flat pictures taken from the Yale chronicles.

7. The Pre-School Child

Interest in the pre-school child grows apace, to judge by the increasing number of books, monographs, and articles in this field.

The large volume by the National Society for the Study of Education on pre-school and parental education (121) heads the long list. Books giving systematic presentations of the psychology and education of the young child are by Bernfeld (11) based on Freudian psychology, by Blatz and Bott (15), by Drever and Drummond (41), by Faegre and Anderson in their revised volume on child care and training (51), by Goodspeed and Johnson (70), by Muchow (119), by Pyle (136) and by Tilson (168). In addition is the chapter by Gesell (65) referred to above. Monographs include the study of the acquisition and interference of motor habits in young children by McGinnis (106), experimental studies in pre-school education by Pyle and Murphy (137), and the study of the smiling and laughing of infants in the first year of life by Washburn (172). Speech development in the early years has been studied by Foulke and Stinchfield (53) and by Haggerty (77). Articles on various problems of early development are by Gesell (64) on maturation, by Gesell and Thompson (66) on learning and growth in identical twins and by Weiss (176) on the measurement of infant behavior.

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INTELLIGENCE TESTS

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General Books. Only one book dealing exclusively with intelligence testing has come to the writer's attention during the past year. This is a German book by Hylla (68) which gives a description of tests suitable for German schools. It includes a translation of the Stanford Revision, and gives a brief summary of the results of testing at Hamburg, Berlin and Leipzig. It describes the first German "Testheft" constructed by Bobertag and Hylla in 1925 following the American model of a group test booklet. The author claims this test booklet to be as good as the longer and more complicated separate tests generally used in Germany. The book gives a general discussion of intelligence, standardization, the I.Q. and elementary statistical techniques. It is written for the use of German teachers.

Thorndike (153) has written the article on intelligence tests in the new Encyclopedia Britannica, and Pintner (127) contributes a chapter on the same subject in the Foundations of Experimental Psychology, edited by Murchison. A chapter on the history of intelligence testing is included in Murphy's (110) general history of modern psychology.

Several books contain one or more chapters on intelligence testing. Brooks (17) deals very thoroughly with the problem of the growth of intelligence, Levine and Marks (86) interpret intelligence in terms of Gestalt and give samples of different intelligence tests, Thomas (152) summarizes the psychometric approach to the study of the child, and Moss (109) emphasizes individual differences in intelligence. Very brief and inadequate chapters on intelligence testing appear in the books by Garrison and Garrison (49) and by Greene and Jorgensen (55).

Bibliographies. Pintner (126) gives the usual yearly summary in this Journal. Louttit's (95) bibliography on sex differences covers intelligence tests.

The Meaning of Intelligence. Hazlitt (58) devotes the major part of her book to a discussion of the meaning of intelligence.

"Intelligence is the problem-solving organization of the mind," and the general factor in intelligence is the measure of the degree to which experiences influence one another or are confluent. She constructs one or two new tests arising out of this idea of intelligence and gives the results of testing some college students. Spearman (147) contributes a brief statement of his theory to the new Britannica, and he (146) also claims that Kelley's results in the "Crossroads" support the Spearman theory. Asher (8) examines intelligence tests by means of the Spearman tetrad criterion. Stern (148) elaborates on his well-known definition of intelligence in a theoretical article on the meaning of general intelligence.

The Relations of Intelligence. McClatchy (96) discusses the concept of social intelligence and finds a correlation of .16 between the George Washington Test and rankings of social adaptability, and a correlation of .53 with the Colgate Introversion-Extraversion Test. Hovey (65) finds very low correlations between Army Alpha scores and three measures of extraversion-introversion tendencies. Oates (115) finds no correlation between intelligence and ratings for temperamental qualities but a correlation of $+.56$ between school marks and temperament (with intelligence constant). He concludes that persistence is the most important factor in determining school success. Hartshorne and May (57) in their extensive study on service and self-control find a correlation of $+.16$ with intelligence. Intelligence and persistence correlate $+.15$; intelligence and self-control is zero. Schneider (140) gives correlations between intelligence tests and the Rorschach Test of interpreting standard ink blots. He believes this test to be better than the usual intelligence tests in measuring the abilities of repressed children, and supports his contention by case studies. Mackaye (100) maintains that intelligence is part of a total organic attitude involving emotional conditions, etc. In cases of emotional instability intelligence is subordinate to the total organic attitude and this must be taken into account in the classification of pupils. Meili (102) discusses the factor of nervousness and emotional difficulty during testing and expects the examiner to counteract such disturbances. Earle (36) gives seven performance tests to 570 children between C.A. 13-9 and 14-1. Detailed results and sex differences for each test are shown. Performance tests do not measure intelligence as well as verbal tests. They correlate .79 with Binet and .60 with a group test. There is no group factor of non-verbal ability and hence such tests should not be combined. Pyle and Snadden (132) compare

high and low I.Q. cases in high school on thirteen different tests. They find an overlap on six tests and none on seven (mostly ideational learning tests), and they conclude that most abilities are very specific and that there is danger in pooling the results of several tests. Conrad and Jones (22) suggest that an intelligence test might be constructed by means of questions based on motion pictures, which would be very serviceable for the testing of adults. They find correlations of .68 and .71 between such tests and Army Alpha scores based on over 100 cases, ages 10 to 54. The age curves for the Alpha and picture test scores are very similar. The reliability of the picture tests is .90 to .93. Bobertag (11) gives three intelligence tests each four months apart to the same pupils and correlates these scores with the teacher's ratings, a new rating being made for each test. The correlation of test with teacher increases from .53 to .66 to .78, while the correlation of test with test does not change. This shows the test rating to be more valid than the teacher rating. Atkinson (9) correlates intelligence scores with scores on learning tests at different stages of the learning. He finds that "intelligence seems to be involved more in the preliminary adjustment to relatively simple situations, less in subsequent achievement and least in the mechanical limits of ability." He differentiates between learning and intelligence, and finds that the more complex a response, the more novel a situation, the more it requires intelligence. In a similar manner Hertzberg (62) finds that motor dexterity alone is of no value for predicting M.A. in his study of the motor ability of 46 kindergarten children. With reference to mechanical ability Kefauver (75) finds little difference between the correlations for 101 cases ranked according to shop work and abstract tests of intelligence and mechanical tests. The coefficient with shop work for the Terman Group Test is .22; for the McQuarrie Test .30; for Stenquist Mechanical I and II .45. Art ability as tested by the Lewerenz (88) Art Test seems to have no correlation with the usual intelligence test. Uhl (157) in a study of 822 high school students finds negative correlations from .01 to .08 between intelligence and time spent on studies; mostly positive correlations with time spent on educative activities out of school; and all positive correlations with time spent on extra-curricular activities. He concludes that "superior students are not taxing their capacities as heavily as slower students."

Growth and Constancy of Growth. Thurstone and Ackerson (155) from a study of 4,208 Binet tests with children from

C.A. 3 to 17 arrive at a growth curve positively accelerated in the younger years with an inflection point between ages 9 and 12 and negatively accelerated thereafter. This extraordinary curve runs counter to all previously suggested growth curves. Odom (118) studies the results of group intelligence tests and arrives at the usual curve showing negative acceleration, but with no sign of an upper limit at C.A. 13 or 14. The shape of the curve of intelligence after childhood is studied by Mursell (111) who takes the results of a great many intelligence tests given to reformatory inmates and classifies them into age groups of 5 years from 10 to 74. He finds a steady decrease in I.Q. from the twenties onward, a decrease from I.Q. 89 or 90 to about 67, the biggest drop being from age group 45-49 to 50-54. With idiots, Moore (105) believes that mental growth ceases about C.A. 10. This is based on re-tests of 51 idiots. The median change in I.Q. per month is about $-.063$. Adams (2) gives a description of a case tested 21 times from 1911 to 1926, showing a gradual decrease in I.Q. from 86 to 54. Woyczynska (178) reports the results for 942 tests of 446 children with the Bobertag revision of the Binet. The interval between tests varied from 6 months to 6 years. The I.Q.'s were mostly below 100. The correlation for these 942 tests is .87; the median difference in I.Q. is .02, and the interquartile range from $+5.8$ to -3.6 . These results are very similar to those found in this country.

Influences upon Intelligence Ratings. Working in the presence of others or in groups has no influence upon intelligence test ratings of university students according to Farnsworth (41). Marine (103) does not find that being familiar with the examiner influences the Stanford I.Q. of young children. To establish familiarity she devoted a certain amount of time to playing with the children; the unfamiliar children she had never seen before the test period. She found no reliable difference in I.Q. between the two groups previously equated for intelligence. Van Alstyne (159) worked with 75 three-year-old children and measured many environmental factors in a fairly objective manner. She found all degrees of correlation between these factors and the intelligence of the children, some being highly correlated therewith. Chauncey (19) gives correlations between Sims' Score of home environment with Stanford Achievement (.30 and .35) and also with Multi-mental (.21 and .19) for 113 children in grades VIII and IX. There is no correlation according to Viteles (162) between score on the Brown University Psychological Test and age of pubescence (first menstruation) in a

report on 236 cases. Westenberger (170) gives intelligence and achievement tests at the end of the first semester and at the end of the school year to 404 children in grades II to VIII. After a thorough medical examination forty per cent of the poor group received medical attention, but no difference in intelligence or achievement is found between the treated and untreated groups. Common minor defects seem to have no influence on intelligence or achievement. Kempf and Collins (77) find that the average number of physical defects decreases with increase in I.Q., and ear defects show much the greatest increase with decrease in I.Q. "The relationship between I.Q. and physical defects appears to be of a general rather than a specific nature."

Scales and Individual Tests. No new intelligence scales for individual testing have come to the reviewer's attention. Kent and Shakow (79) describe a revision of their previously published Worcester series of form-boards. Kovarsky (82) presents a revision of the Rossolimo tests. She reports that Rossolimo first described his tests in 1909. His method is an analytic method as opposed to the synthetic method of Binet. In the Kovarsky revision there are 28 series of tests divided into tests of attention, will, memory, judgment and so forth. The testing of a case takes from $2\frac{1}{2}$ to 5 hours. Each series is scored on a ten-point scale and drawn on a profile chart. The equation for the profile $(P) = t$ (attention) $+ r$ (memory) $+ s$ (higher capacities). The profile for imbeciles lies between $P=2$ and $P=4$; that for morons lies between $P=5$ and $P=8$. Types of children are distinguished according to the relationship between the levels of the three different terms of the equation. An analysis of the responses to the Healy Pictorial Completion Test II is given by Dorcus (33), and Remmers (136) shows how the mean M.A. for the different parts of the Herring Revision fluctuates from part to part in a study of 101 freshmen tested on this test.

Group Tests. Kent and Shakow (78) present a standardization of several tests which can be combined by the median M.A. method and are specifically designed for clinical use. Schultz (141) gives the Viteles T 100 test to 392 cases in grades VI to VIII and finds a reliability of .91 and a validity between .80 and .91. He finds the test a useful one for a vocational bureau. Vance (160) describes a reasoning sub-test of the Iowa modified Alpha for college students. Doll (32) describes three performance tests used for group testing of illiterates. Walters and Thomas (166) present a standardization

of Spearman's measure of intelligence for schools, and Piéron (123) takes a French translation of Mira's Barcelona Test, gives it to 3,017 subjects, ages 13 to 20, and presents decile norms for each age and sex. She finds the normal school students slightly superior to the lycée students. The scores increase from age 13 to 20 and she concludes that intelligence increases with age among adolescents who continue in school. Two studies compare primary intelligence tests. Sangren (139) studies seven such tests with 100 Grade I children. The tests are ranked as to nineteen criteria and the final ranking is Haggerty Delta 1, Pressey, Pintner-Cunningham, Detroit, and so forth. McGraw and Mangold (99) use ten first grade tests given in 1923-4 and correlate them with the Otis Group given five years later to the same children. Dearborn correlates .60, Pintner-Cunningham .55, Otis Primary .53, Haggerty .53, and so on down. They also present intercorrelations of all the tests and correlations of each test with the average of all others. Witty and Taylor (175) compare the Multi-mental with the Binet M.A.'s of 522 children in Grades IV to VI, and find a higher correlation for the Multi-mental with the Stanford Achievement than for the Binet. Oates (116) gives three English group tests to 270 boys, age 11 to 18, and presents intercorrelations, as well as correlations with teachers' estimates (from $-.32$ to $+.71$, by grades) and with school examinations ($-.10$ to $+.62$, by grades). An examination of the sub-tests by the ratio of the young bright to the old dull leads the author to suggest that the best sub-tests for innate intelligence are Number Series, Absurdities, Best Reason, Cipher, Analogies and Common Sense; whereas the poorest are Mixed Sentences, Orientation, Obeying Orders and Meanings of Words. Two studies point out that the significance of I.Q.'s may vary from test to test according to the method of standardization. Kefauver (74), therefore, constructs a table of equivalent I.Q.'s for ten group tests, and Cole (20) gives T scores for three well-known group tests.

The School Child. Purdom (131) presents a study of homogeneous grouping, and finds that such sections do not gain more either on standard tests or on semester grades. Furthermore, no degree of intelligence shows an advantage with homogeneous grouping. And Bonar (12), likewise, in a smaller study finds no difference in reading tests at the end of one year between a high I.Q. group (av. I.Q. 106), a low I.Q. group (av. I. Q. 97) and a mixed I. Q. group (av. I. Q. 98). Lincoln (93) summarizes all the published results on homogeneous grouping and finds that no effect can

be expected unless the teaching is modified. One group intelligence test is not sufficient for ability grouping. Kefauver (73) discusses various ways of sectioning according to ability, and advises a composite of (1) teachers' estimates or marks, (2) intelligence tests, (3) achievement tests. Abelson (1) compares the A.Q.'s of six groups in Grade VI under the Dalton Plan, and concludes that this plan is probably good for dull pupils. Mort's (108) treatment of pupil management in school includes a discussion of the use of intelligence and achievement tests for classification, guidance, promotion and the like.

Lincoln (91) compares the standing of nine schools on intelligence and educational tests and finds a high correlation between them. Van Wagenen's (161) extensive survey of six groups of schools with both intelligence and achievement tests gives comparisons of each subject with mental age constant. In general he finds that the length of the school year has a decided influence on school achievement. The nine-months' school is better than the eight-months' school by about one-third of a year in achievement. Kempf and Collins (77) report the I.Q.'s of 5,000 children in two very different Illinois counties. The correlation between I.Q. and school marks for 3,747 cases is $+.39$. The median I.Q. for disciplinary cases is 95 ($n = 400$), for non-disciplinary cases 101 ($n = 4,288$). The median I.Q. for the northern county is 102, for the southern 88. In the northern county the urban I.Q. is 103.5, the rural I.Q. 95; in the southern county the urban I.Q. is 91, the rural I.Q. 84. Urban native whites in one county obtain a median I.Q. of 108; rural native whites 99. Wood's (176) report of testing in private schools in the East agrees with several previous studies, showing a median I.Q. of 118 for 1,157 children and this is contrasted with a median I.Q. of 98 for 1,057 city public school children. Gerberich and Stoddard (51) give some results of a survey of Iowa High School Seniors. They show that the upper decile in intelligence is made up of 887 cases, of which 59 per cent are male and 41 per cent female, and, furthermore, the age distribution of this group shows a much younger C.A. than that of the total group. Their test battery and first semester grades correlate from $.50$ to $.54$. In England Marsden (101) gives the results of Binet and group tests in remote country schools in Yorkshire and the South-West of England. He gives a distribution of I.Q.'s for the 197 cases tested. He finds that schools differ very much in reference to the total number of superior children. Comparing his results with the Northumberland

survey, he does not find as many superior I.Q.'s. In Germany, Giese (52) gives the results of 8,000 children tested by means of group intelligence tests and compares different types of schools. He finds the Volksschule has the lowest mean rating. He finds a general increase in score by age from 14 to 17. He finds no sex differences. He finds differences between towns and between different courses of study leading to different occupations.

The College Student. Edgerton and Toops (37) make an important contribution to the use of intelligence tests in college administration. Of 1,958 freshmen entering Ohio State University in 1923 only 18 per cent graduated within four years and probably only 34 per cent will ultimately graduate. A very large proportion of those with poor intelligence ratings did not graduate in four years. The intelligence test correlates .45 with scholarship; intelligence plus high school record correlates .56. The best ten per cent in intelligence earned 1.78 as many hours credit and 2.46 as many honor points as did the poorest ten per cent. The authors believe that the university is not securing the attainment from the superior student that is clearly possible, and this is particularly true of women students. Hammond and Stoddard (55) give detailed results of the Iowa Training and Aptitude Tests given at the University of Iowa. Crawford (24) finds very little difference between the mean score or S.D. of those who were candidates and those who were admitted to Yale on the scholastic aptitude test. The correlation between intelligence and time spent in study is $-.20$ ($n = 1,166$). He presents the test ratings according to occupation of parent, showing professions highest and business lowest. Brigham (15) writes the fourth annual report on the scholastic aptitude tests, discusses item analysis and the validity of the test. The correlation with marks is .51 to .55 ($n =$ about 300). Kellogg (76) contrasts intelligence tests and matriculation examinations with reference to their correlations with students' marks. Eells (38) finds that students entering Stanford from junior colleges score higher on the Thorndike Test than the regular Stanford student. Dexter (31) finds that students majoring in English have a higher average intelligence score than those majoring in other subjects, and West (169) gives the results of testing the students at Battle Creek College. Freeman (45) interviews 68 sophomores who are high on tests and low on grades or vice versa, in order to find the factors that reduce correlation, and Jones (69) studies 40 students scoring low on Army Alpha, 82 per cent of whom failed to maintain an average mark of C. He finds

such students poor college risks. Constance (23) compares fraternity and non-fraternity freshmen, and finds no difference on the intelligence tests, but finds that the former secure slightly better college marks.

There are several reports of intelligence testing in normal schools and teachers' colleges. Frasier and Heilman (44) discuss the general use of the intelligence test and present some results. They say that the intelligence score is the most valuable single item in college administration. Four studies deal with the correlation between intelligence and teaching success. Pyle (133) finds that intelligence and college marks correlate .45, but intelligence and practice teaching only .15; and, furthermore, when teaching success is based on the judgments of principals in actual school work this correlation drops to .03 for the first year, and .02 for the second year of actual teaching. Sorenson (145) discusses Pyle's findings and calls attention to the homogeneity of his groups and the unreliability of the principals' ratings. He presents some data of his own, where the Otis I.Q.'s range from 86 to 123 with a mean I.Q. of 107.5. The correlation with age is $-.62$ (ages 16 to 55). But Frasier (43) analyzing the results of hundreds of normal school students finds no relationship between teaching grades and intelligence. There is no difference in the teaching grades of the top five and the bottom five per cent in intelligence. For 70 cases, Alpha and teaching grade correlate $-.03$. On the other hand, Broom (18) with 148 cases finds a correlation of $+.30$ between grades in practice teaching and the Thorndike Intelligence Examination, and this is higher than the correlation between teaching grades and grades in theory courses ($r = .21$), and he believes in the intelligence test for the determination of a critical score. Condit (21) reports the results of 559 freshmen and finds a correlation of .45 between academic grades and intelligence scores, and .50 between academic grades and achievement test scores. Cuff (26) finds that intelligence increases from one college year to the next because of elimination of the less intelligent; 22 per cent of those eliminated fall below the 25th percentile in intelligence, as compared with 17 per cent of those retained. Lauer and Evans (83) report correlations of an intelligence test with high school subjects, and Sims (143) contrasts normal school freshmen with college freshmen on various tests and deplores the poor intelligence, achievement and socio-economic status of the former. Kesselring (80) in Germany discusses entrance examinations for teacher training schools. He presents results for his own examina-

tion and for the Bobertag-Hylla adaptation of the Otis S.A., and criticizes the latter. It correlates lower with educational tests and teacher ratings, but it takes only 45 minutes, whereas his examination takes $1\frac{1}{2}$ hours.

The Superior. Danielson (28) gives a distribution of the I.Q.'s of all children in special classes for the superior in Los Angeles. The average I.Q. is 134, and 23 to 27 per cent are above I.Q. 140. Lincoln (92) studies 54 under-age children admitted to school on the basis of intelligence tests now that they have reached grades IV to VII. On standard educational tests they are $+.33Q$ above the median. The bright are especially good on reading and poorest on arithmetic. Similarly, Washburne and Rath (167) find that bright young children admitted to school early have kept up with others in all studies. During six years they have shown less retardation and hence they become increasingly under-age. The writers find the policy of admitting under-age bright pupils to be fully justified. Working with children of I.Q. 150 and above in special classes Danielson (27) made a definite attempt to enrich their reading and found that their A.Q.'s rise over a two-year period. They come up to their intellectual expectation when given the chance. Kiefer (81) compares high and normal I.Q.'s on five motor tests and finds no difference between the means for ages 9, 10, and 11, and low correlations between these tests and intelligence.

The Feeble-minded and Problem Cases. Lewis (89) writes Part IV of the British Report of the Mental Deficiency Committee dealing with the incidence of mental deficiency. Group tests for a first selection were followed by Binet and educational tests, and age 14 was used for the calculation of I.Q.'s of those aged 14 and above. Only those below 60 I.Q. were regarded as feeble-minded. The total defective children in the six areas studied was 0.85 per cent; urban 0.67 and rural 1.05. The incidence for idiots was 30 per cent higher for boys than for girls. The estimated incidence for the total population of Great Britain is 0.73 per cent. This is about twice that reported by the 1906 Commission. This is due to a more thorough survey and to increased longevity, but also, the author believes, to an increase in birth rate of the mentally deficient in rural areas. The percentage of idiots has also doubled, and idiots would not likely have escaped the 1906 survey. The present report undoubtedly represents the most thorough study of the incidence of mental deficiency over a large area. Popenoe (128) in the United States tries to estimate the percentage of the population having an I.Q.

of 70 or less, basing his findings on various surveys of schools, and the like. He concludes that 4 per cent fall below I.Q. 70. Contrast this with the British figure of 0.73 per cent below I. Q. 60, mentioned above. Popenoe points out that his estimate would mean about five million feeble-minded in the U. S. A. and only sixty thousand of these are at present in institutions. Wallin (165) gives a report of all types of special class children in the schools of Baltimore. The Binet results for 201 white and 79 colored show 24.3 per cent of the former and 73.4 per cent of the latter have I.Q.'s below 68. Town (156) summarizes the results of five years' work in a psychological clinic. Of 2,536 children tested, 11.9 per cent were feeble-minded. Of 695 behavior problem cases, 24 per cent were feeble-minded; of 75 unmarried mothers, 29 per cent were feeble-minded. Ellis (39) gives the results of intelligence tests at a feeble-minded colony, and finds a range of M.A.'s from 2 to 12, with the median at 7.

Compiling the results of all cases brought to a clinic, Bridgman (13) finds that out of 3,675 cases, 1,940 were males and 1,735 females. The distribution according to degree showed slightly more male idiots than female (1.5 vs. 1.2 per cent), but more female imbeciles than male (6.9 vs. 5.7 per cent), and a much larger percentage of female morons. Morgan's (106) study of the age of menstruation of 138 feeble-minded girls shows them to be somewhat retarded. Brousseau and Brainerd (16) have written a book on Mongolism, including all information about this type of feeble-mindedness. They discuss the many hypotheses regarding causative factors and reject age of mother and last born in family, and suggest as the causative factor an obscure disturbance of the ductless glands. For 206 mongols the I.Q. mode is about 25. They classify their cases as 38 per cent idiots, 61 per cent imbeciles and 1 per cent morons. For 165 cases the average age of death is 14 and the oldest was 41½ at death. Otis (120) formed a reading circle for a group of feeble-minded girls, ranging in I.Q. from 53 to 85, with a median at 68. She found that most of them showed improvement, and this increased their I.Q.'s on a later Binet test, due particularly to increase in the vocabulary test. Yepsen (179) gave the Goode-nough Drawing Test to 37 feeble-minded boys three times and found re-test coefficients of .89 and .91. The correlation with Binet was .60. Anderson (4) asked teachers of special classes to classify 125 cases as stable or unstable and found no marked differences in intelligence or achievement. Lincoln (94) finds that 502 problem

children brought to a clinic range in I.Q. from 20 to 110 with a median at 75. McClure (97) sent a questionnaire to teachers to list all their problem cases. Out of an enrollment of 26,364 children, 533 or 2.02 per cent were reported. The mean I.Q. was 90.1 for 499 usable cases, made up of 416 boys and 83 girls. The author compares the high and low I.Q.'s as to types of misconduct.

Rabinovitch and Rossolimo-Savitch (134) give the results of a comparison of Binet and Rossolimo tests of 355 abnormal children. There were 183 morons, but according to the Binet test 96 tested normal while none tested normal according to the Rossolimo test. The correlation for these cases between the two tests was .48. Of 46 imbeciles some tested normal according to the Binet. The authors are very critical of the Binet Scale. They regard it as good for a first preliminary classification, but not for final diagnosis and of no use for showing special deficiencies, as is the case with the Rossolimo Test. Geissler (50) reports the use of the Rossolimo Test given according to Bartsch's method to 15 cases in his special class for speech defectives. He gives profiles for each child, a class profile and a class equation, *e.g.*, $P = (5.86 + 7.82 + 7.52) + 24\% V$. Garfunkel (47) finds a larger per cent (96%) of eidetikers among special class children than among normal (65%). There is no correlation between intensity of eidetic imagery and the intelligence scores on the Pintner-Cunningham Test.

The Delinquent. Healy et al. (60) in their recent book find that 85 per cent of delinquents of normal mentality and personality were successful in foster homes, whereas only 40 per cent of delinquents of abnormal mentality and personality were successful. Wiegmann (171) in Germany contrasts 227 delinquents with 227 non-delinquents of the same age and some equivalence of schooling. He gives many intelligence tests and analyses the results of each test separately, finding the delinquents in general below the non-delinquents. Oseretzky (119) presents the results for 378 juvenile delinquents in a Moscow "workhouse." They are below the norm in physical measurements. On Binet Tests only 7 per cent test at age, the rest are one or more years retarded. Twenty and eight-tenths per cent are feebleminded according to the author's diagnosis. In general 62 per cent are physically and 43.1 per cent are mentally backward. Bridgman (14) describes four cases of murder brought before the juvenile court since 1914. The I.Q.'s are 71, 75, 85, 96. The author gives detailed case studies and concludes that two were influenced largely by envi-

ronmental factors, and two by native traits. Riddle (137) presents 343 cases of juvenile stealing, and gives the median M.A.'s and I.Q.'s for different kinds of theft. Poull and Montgomery (129) believe that the Porteus Maze Test shows a difference between socially adjusted and maladjusted cases. The latter do poorer on the test. They are lacking in planning ability. But strangely enough they do not score poorer on Healy Completion II. Mohr and Gundlach (104) study 55 native white adult prisoners. They are superior on the Army Alpha to the Illinois draft, and resemble very much Murchison's sampling of adult prisoners. They give the correlations between intelligence and various physical measures. Raphael et al. (135) give a distribution of the I.Q.'s of 100 cases of traffic offenders. The median is about 80, with a range from 46 to 118.

The Deaf and Blind. Upshall (158) makes a very careful comparison of deaf pupils in day schools and in institutions by means of 311 pairs matched for C.A. and intelligence score. Day schools attract the brighter pupils. They are better on educational tests, amount of hearing, age of becoming deaf and years in hearing school. Yet even when all of these factors are equated the day school is still slightly superior to the institution on educational achievement and the author is inclined to suggest that the teaching may be better. Pintner (124, 125) discusses the relation between measures of speech, speech-reading, intelligence and achievement with deaf pupils. He finds no correlation between the first two measures and the Pintner Non-Language Test, but fair correlations between them and the educational test. Speech is also positively correlated with amount of hearing and age of becoming deaf. Drever (35) in England reports briefly the results of testing 1,474 deaf children, ages 5 to 16, on his performance scale. The age medians for these deaf children are actually a little above the hearing norms which the author says were based on 200 hearing children "very markedly above the normal." He finds the boys decidedly better than the girls on his performance tests. A very wide and detailed research program for the deaf and hard of hearing has resulted from two conferences under the auspices of the National Research Council and this has now been printed (Anon. 7).

Only one reference to the blind has come to the writer's attention. Hayes (59) describes his new revision of the Binet Scale for the Blind. It is standardized on blind children. Their median attainment is below that of the seeing, in terms of I.Q., probably about 10 points below.

Racial Comparisons. Several studies involving negro and white comparisons have been published. Peterson and Lanier (122) report comparisons in Nashville, Chicago and New York on several intelligence tests. There is a marked difference between Nashville whites and negroes on three group tests (2.2 Q), but no real difference on three ingenuity tests (0.8 Q). There is no difference between the New York whites and negroes but a great difference between Nashville negroes and New York negroes. The correlation between skin color and intelligence is about +.21. The authors are critical of our present tests as measures of race differences. Young (180) compares 323 whites with 314 negroes in Grades III and up, ages 9-10, in Louisiana. The children came from the same wards of the city. On the N.I.T. the white score is 70 as compared with the negro score of 40. The light negroes score 45 and the dark 37. On suggestion tests the negroes are about $1\frac{1}{3}$ times more suggestible than the whites. In Kempf and Collins' (77) large survey in Illinois the negro I.Q. is 71 as compared with the white I.Q. of 88. Price (130) gives results for several different intelligence tests in eleven negro colleges. The percentage of negroes exceeding the white median varies from 9 to 43 for 7 negro colleges on the Otis Test. In terms of I.Q. the white median is 109 and the negro 98. Davenport and Steggerda (29) write on race crossing in Jamaica. They are mainly concerned with anthropological measurements but they report briefly results on some psychological tests. The number of cases varies much from test to test and little is said about the selection of cases for each test. In general they conclude that the negro is superior in sensory equipment, but the white is far ahead in planning. Gray and Bingham (53) in a report devoted chiefly to musical ability tests, give results for 258 colored and 219 white on an intelligence test. The average index of brightness of the colored is 76 as compared with 108 for the whites. Incidentally it may be noted that these authors find the whites superior on musical tests, whereas Davenport and Steggerda, mentioned above, found the negroes superior. Viteles (163) gives a summary of test comparisons between negroes and whites, but presents no new data.

Hughes (67) gave intelligence and achievement tests in three London elementary schools with a mixed Jewish and Gentile population, and found the Jewish children at almost every age from 8 to 13 superior to the non-Jewish in both intelligence and achievement. When classified according to parental occupation the Jewish excelled the non-Jewish by 6 to 10 points of I.Q. in most groups. Similarly

Garrett (48) in this country studying 296 college freshmen found the Jews superior to the others on the Thorndike Intelligence Test, the Moss Social Intelligence Test and in college marks. After the Jews, on the Thorndike Intelligence Test, came the German, English, Italian and lastly the Irish. The Italians were last on the Moss Test and the Irish in college marks. The Thorndike and Moss Tests correlate .57, the Thorndike and marks .63. Lima (90) reports the results of group tests given in seven towns in Minnesota and compares 10 nationalities, comprising 85 per cent of the population studied. The average I.Q. is 107. The English, Danish, Scotch, Swedish, Norwegian rank in that order above the average. The Dutch are at the average. Below come the Irish, German, Finnish and French. No nationality comprising less than two per cent of the population is included in this list. The Illinois county survey by Kempf and Collins (77) gives the following average I.Q.'s for nationality groups: native white 107, mixed 105, British 105, Scandinavian 104, German 98, Polish 87, Italian 87. Lester (85) gave six performance tests to 26 foreign children in Grade I and found correlations ranging from .28 to .65 with the Binet and from —.05 to .54 with teachers' estimates. Polish children did better than American on the Kohs, Knox Cube and Healy A, and he advises a combination of the two latter tests with the Binet for foreign children. Hsiao (66) reviews all the studies of Chinese and Japanese compared with Americans. He lists the tests on which each nationality is superior. Wood (177) gives the results of 120 women students in Constantinople Women's College. The mean I.Q. on the Otis Higher S.A. is 95.5 as compared with a mean of 118 reported by Otis. The author gives separate means for four different nationalities.

Inheritance. Wingfield (173) gives results for 102 twin pairs, (45 identical and 57 fraternal) and 29 orphans reared together for 25 to 75 per cent of their lives. The correlation for all twin pairs is .75 (age constant), and the correlations for older and younger pairs are .71 and .77 respectively. The fraternal pairs correlate .70 and the identical .90. For 15 pairs of orphans the correlation is .13. Twins are as much alike on educational as on intelligence tests, hence schooling does not make them more alike. Newman (112, 113, 114) has three articles each dealing with a pair of identical twins reared apart from early life. In general the twin having had better education and better home environment obtains a higher I.Q. to the extent of 12 points. Holzinger (64) gives cor-

relations for mental and physical traits of 50 identical twin pairs (average $r = .90$) and for 50 fraternal like-sex twin pairs (average $r = .60$). He finds nature and nurture equally effective in producing mean twin differences in intelligence. McFadden (98) finds that the I.Q.'s of siblings on the Terman and Kuhlmann tend to decrease from the youngest to the oldest child. There is no such decrease on the N.I.T., because it is too dependent on schooling.

Freeman (46) discusses the general nature-nurture problem and finds it impossible to separate nature from nurture. Paterson and Williamson (121) take Pearl's own data and, by using the Barr and Taussig scales, show that his so-called average social status is in reality much above the average, and, therefore, the data confirm the findings of Galton and Terman instead of contradicting them, as Pearl had argued. Allen (3) investigates the background of 49 families containing gifted children (range of I.Q.'s 133-190, mean I.Q. 157). Seventy per cent of the fathers are in the highest group of Taussig's classification. The score on the Whittier Home Rating Scale is 23.3 as contrasted with 20.8 for unselected cases. Ten cases of insanity were found in the 2,800 forefathers reported, or about the same rate as in the population at large. The author finds that "these families are not being maintained by reproduction." Willoughby (172), however, finds that college stocks are in no danger of elimination, although there is a small negative correlation ($r = -.3$) between intelligence ratings and fertility. Sutherland (150) pushes further the relationship between I.Q. and size of family by investigating children of fathers all in the same occupational group, *i.e.*, miners. With 3,096 cases he reports negative correlations for different samples, $r = -.13$; $-.14$ (girls only); $-.12$ (boys only). Hence, when occupation and social status is about the same, we find the same tendency for large families to have lower I.Q.'s, as we find in populations of mixed occupations and social status. Thurstone and Jenkins (154) find a correlation of $-.09$ between intelligence and size of family. Their study includes 10,000 case records of a clinic, ages 1 to 21. The distribution of I.Q.'s for birth order shows that the mean intelligence rises with order of birth and that this effect seems to be progressive as far as the eighth child. But Jones and Hsiao (71) find no difference in intelligence due to birth order in a study of 614 pairs of siblings by calculating the mean difference in sigma score for adjacent pairs and non-adjacent pairs. Stroud (149) reports a correlation of $-.16$ for intelligence and size of family in a study

embracing 1,057 cases, ages 5 to 18, in rural regions in Georgia. He further finds a correlation of $+.26$ between intelligence and the tax assessment of the home, in spite of the homogeneity of the group in both variables. In Russia, Sirkin (144) gave group intelligence tests with a reliability of .96 to from 6 to 8 hundred children in each of Grades IV, V, and VI and found a rise of score with social level. His correlations for score and level for the three grades are .36, .43, .39. A second testing 14 months later gave similar results. Witty and Lehman (174) compare the heredity background of 50 cases I.Q. 70 and below with 50 cases I.Q. 140 and above. The parents of the low I.Q.'s are 78 per cent American, mostly laborers with no college education; the parents of the high I.Q.'s are 98 per cent American, mostly in business and 50 per cent have college education. The authors conclude that such results may be due to either hereditary or environmental influences. Heilman (61) in a study of 828 ten-year-old children finds that 57 per cent of the variation in E.A. is due to M.A., 7 per cent to environment and the rest not accounted for. The maximum amount of variation in E.A. due to school training is 19 per cent, and the remaining 81 per cent is perhaps due mainly to heredity. But only 2 per cent of variation in grade location is due to M.A. and about 31 per cent to school attendance.

Employment and Guidance. An anonymous article (Anon. 6) describes the uses of intelligence tests in personnel work and gives descriptions and norms of suitable tests. Anderson (5) in his book touches on the use of intelligence tests in a large store, finds a slight correlation with sales ability, and gives a percentage distribution of the I.Q.'s of 500 sales clerks. In the dull group (I.Q. 80 to 90) we find 34.4 per cent, and 19.6 per cent fall even below this, whereas only 5.4 per cent rise above I.Q. 110. Scudder (142) gave the Terman group test to 264 veterans being trained as accountants and bookkeepers. The median score for those finishing the course was much higher than for those who dropped out. After three to four years of employment 103 cases were followed up. Those scoring high were doing better than those scoring low, and this was true also of the average monthly increase in wages. The author suggests minimum scores for these two occupations. Walch (164) gives a survey of the literature on mental tests used in guidance with a bibliography of 74 titles. Roloff (138) in Germany discusses the use of intelligence tests in industry and the problems of validation of such tests. Hartson (56) gives the intelligence ranking of the

occupations of 1,135 alumni of Oberlin. These rank from college teaching and secretarial work (high) through medicine, law, and so forth, down to art and physical education (low).

Miscellaneous. Odell (117) discusses briefly the Achievement Quotient and decides it is too unreliable for use. Dearborn and Smith (30) re-scored 530 test blanks and found that 73 per cent contained errors, 6.4 per cent of which amounted to an error of one-half or more years of mental age. They analyze the different types of errors. Dougherty (34) presents case studies of 18 children tested and treated at an educational clinic. Wells (168) discusses testing in relation to many medical problems. Fracker and Howard (42) present correlations between the six Seashore music tests and intelligence for college students. Pitch and I.Q. correlate highest ($r = .32$). Hindman (63) finds that 155 athletes out of 1,327 freshmen college students fall below the nonathletes by 2.76 percentile points and thus show no difference in intelligence. Lewerenz (87) finds that high I.Q.'s do better than low I.Q.'s in visual education lessons, but the low I.Q.'s profit relatively more from such instruction. Kaulfers (72) finds a general rise in achievement in Spanish with rise in I.Q. The difference between the sexes is marked. Boys require an I.Q. about 10 points higher to equal the achievement of girls. In comparing 500 pairs of husbands and wives, Jones (70) finds a correlation of .25 for physical traits, but a correlation of .55 on intelligence tests. He sums up all data so far published on this topic. Symonds (151) discusses the choice of items on the basis of difficulty. Awaji (10) reports the results of testing 6,000 Japanese soldiers, probably a random sampling of Japanese males because of compulsory service. He shows the curves for each of the nine subtests and almost all approach the normal. The order of occupations is the same as found in the American army. He gives also distributions according to school training and arm of service. Estabrooks (40) takes 444 children of North European stock and finds no correlation between pigmentation of eye and hair and intelligence. Crosland et al. (25) find no relation between intelligence and amount of error or amount of improvement resulting from practice on the Müller-Lyer illusion. Moriwaki (107) takes 15 students and finds a correlation of .28 between their intelligence scores and the average rating of their intelligence from their photos by four judges. The correlation of intelligence score with rating of intelligence from a short interview goes up to .56. Lehman and Wilkerson (84) analyze the results of a play questionnaire given to

6,000 cases and find that C.A. is more potent than M.A. in influencing a boy's play behavior.

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EDUCATIONAL TESTS

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I. GENERAL

Since it was felt that most readers will be interested in the developments in particular aspects of the measurement work, this review has been organized as nearly as possible around the different fields of activity. New tests which have appeared are therefore not all grouped together, but are discussed in connection with the use to which they are to be put. The most noteworthy developments this year have been in the refinement of diagnostic tests and in the construction of unit tests. This conclusion is based on a comparison of the number and significance of articles in these fields this year with those of previous years.

Three general textbooks on educational measurements have appeared within the past year. Greene and Jorgensen (54) give a clear exposition of the various types of tests and the principles governing the selection and application of them by the classroom teacher. This book is written from the point of view that the most important function of testing is the diagnosis of pupil difficulties and the improvement of teaching, and much material illustrative of this type of work is included. A comprehensive handbook on educational test procedures has been prepared by Russell (117). This book gives the most complete and detailed account of the method of computing various scores in the age, grade, and T-score scales that has appeared in one volume. Carroll (24) presents an elementary handbook on measurement, which contains one chapter on methods of handling certain selected educational tests. Pintner (107), in his new textbook on educational psychology, makes an important contribution in calling attention, through his organization, to the close relation of educational measurement to other work in educational psychology. He takes the attitude that some of the most important of the direct contributions to this branch of psychology have come through the work in measurement. In his text he includes three chapters on educational tests as an integral part of his general organ-

ization, and not as a sort of addendum which has been quite a popular practice.

A general but condensed treatment of tests is given by Freeman (47) in the "Foundations of Experimental Psychology." This chapter covers the history, construction, standardization, use, and evaluation and interpretation of educational tests considered as measures of special abilities. A bulletin by Monroe, Odell, and others (91), containing a lengthy review of educational research during the years 1918-1927, has appeared. One chapter is devoted to research in educational measurements. An extensive bibliography constitutes an important part of the work. Jones (76) gives a review of the developments in the field of educational tests during the year 1928. The bibliography includes 121 titles. Odell (98) has made an analysis of twenty recent publications on educational measurements and compiled a frequency table of references to outstanding contributors in that field. The men referred to with the greatest total frequency are Thorndike, Ruch, Terman, Monroe, and McCall.

Sangren (121), in discussing the need for more adequate measures in arithmetic, points out the present trend towards more analytic and diagnostic types of test, of which the Wisconsin Inventory and the Compass Diagnostic Tests are examples.

II. DEVELOPMENT AND USE OF TESTS FOR SURVEY PURPOSES

(a) *New Tests.* The most important battery of tests for the elementary school level to appear during the year is the new Stanford Achievement Test, which was devised by Kelley, Ruch, and Terman (78). It is a complete revision of the earlier battery. Ten tests covering the major school subjects are included in the booklet for grades 4 to 9. A separate booklet containing five tests has been made for grades 2 and 3. Three equivalent forms of these batteries are now available and two more are promised. Age and grade norms are provided. The standardization of this series has been done very carefully and thoroughly. Another significant battery to which we wish to call attention is one prepared by Sones and Harry (127) for use in high schools. It provides a measure of general achievement in four fundamental fields—language and literature, mathematics, natural science, and social studies. Two forms are available, and they correlate with each other up to the extent of about .90. Norms for each semester are given in percentile units.

As for new instruments in special fields, we may mention first a number of tests which have been designed for measurement of reading. A first grade reading scale has been prepared by Cutright, Van Wagenen, and others (32). Only tentative norms are available at present. A first grade word reading test has been constructed by Pressey (111) in forms A and B. It consists of words chosen from the Gates Primary Word List. The two forms correlate to the extent of .93. Norms are being determined. Williams (151) has devised a test to measure comprehension in reading for grades 3 to 9. Age and grade norms are given, but so far no facts have been presented on reliability and validity. Payne (104) reports norms for reading of passages from Gray's Oral Reading Paragraphs presented tachistoscopically for .1 second. The norms are based on 400 pupils in grades 2, 3, 4, and 5. Weeks (148) describes a vocabulary test which has been under construction for a number of years. Reliability coefficients are in the neighborhood of .90. Distributions of scores by ages and grades are given from which norms can be determined. A vocabulary test, called the Centennial Test of Word Meanings (66), has been worked out by Hill in connection with Italian-American children. It contains words from Gates's lists.

Several new tests and scales have been constructed for use in measuring language abilities. The Wilson Language Error Test (153), designed to aid the pupil in recognizing and avoiding the common language errors, has been revised. Stoddard (134) describes a new test of English grammar used in a survey of forty high schools in Iowa. The items were chosen from other tests, agreed upon by judges, and given a preliminary tryout on 200 students. Reliabilities by grades range from .87 to .93. Complete norms are given. Results show large variability among schools. Davis (35) has devised a test of English fundamentals. Burch (20) has constructed a test which purports to measure comprehension of literature under the following three rubrics: action and event, character portrayal and emotional appeal, and intellectual interest. The test is suitable for junior and senior high schools. Grade norms are available and some facts on reliability are given. Huxtable (70) presents some preliminary results on the construction of a scale for judging the thought content of written English which is being designed for use in high school and college. Daringer (33) describes a test of ability to make topical outlines. Norms based upon 400 subjects are presented. Two forms are available, which correlate to the extent of .90 with each other. The Guy Spelling Scales (59)

designed for use in grades 2 to 9 have appeared. Three forms are available and grade norms are provided for each.

There are many other subjects in each of which only one or two tests have appeared. The Columbia Research Bureau Algebra Series is now supplemented by the publication of a new section, called Test I (159), which is intended for use at the end of the first semester of that subject. The average self-correlation of this test is .92, and its average correlation with teachers' marks is .70. Percentile norms based upon 598 students are given. A chemistry test by Wood and others (158) appears in the Columbia series. It covers elementary work in high school and college. Percentile norms based upon 8,000 cases are given. Facts on reliability and validity are also presented. Two tests in plane geometry have been constructed by Orleans (102), one for the middle and the other for the end of a year's course. Percentile norms are given. Scores on each test correlate about .80 with teachers' marks. The median of the self-correlations obtained on Test I is .85, on Test II, .71. The Junior American History Test by Carman, Barrows, and Wood (157) is a comprehensive achievement test in American history designed primarily for junior high schools. Percentile ranks and reliability figures based upon seventh grade pupils are given. Powers and Oakes (95) have constructed an important test in general biology for use in secondary schools. Coefficients of reliability average over .90. Norms are available. Elwell and Fowlkes (43) have devised a bookkeeping test in two parts, one for each semester. Norms are based upon more than 200 high school students. The reliability coefficients for the first and second parts are .82 and .87, respectively. Conard (30) describes the construction of a manuscript writing scale. It consists of twelve forms for use in judging pencil-written specimens in grades 1 to 4 and ten forms for use in judging pen writing in grades 3 to 6. Samples were selected by successive groups of 40, 26, and 10 judges. West (150) has devised a handwriting scale which is especially applicable to the Palmer system of writing. It contains samples scaled in both rate and quality for grades 2 to 8. Grade norms are given. Bathurst and Scheide-mann (7) describe a test of knowledge of general psychology designed for use in college classes. Norms based upon about 400 students in various colleges are reported. The reliability of the test is .80. Odell (99) reports an experiment with a scale designed to assist in the evaluation of answers to thought questions in four high school subjects. He found that ratings with the scale were only

slightly more reliable than ratings without it. Van Wagenen (144) has published a reading scale in educational psychology, with tentative norms derived from a class of college juniors.

A very significant extension of measurement to a new field is represented by the construction of the Health Education Tests described by Franzen (45). This represents the first application of the more refined methods of test construction to the field of child health. Norms are available for grades 5 and 6. Tables for obtaining standard deviation indices are given separately for boys and girls and for five different intelligence levels for each part of the test. The reliability as determined on eleven-year-old children is about .86.

An art judgment test for use in junior and senior high schools has been prepared by Meier and Seashore (88). The test consists in having the subject choose the better in each of 125 pairs of pictures. Tentative norms are provided.

Additional tests which have come to our attention are as follows: a physics test by Miller (90); a test of knowledge of laboratory technique in chemistry by Persing (106); a series of tests for mechanical drawing by Badger (5); a Latin grammar test by Hutchinson (69); a French test by Sammartino and Krause (120); an achievement scale in household science by Davis (34); a clothing test by Frear and Coxe (46); a general business training test by Smith (126).

(b) *Use of Tests in Studies of a Survey Nature.* Eels (42) reports the frequency with which different school subjects were tested and the frequency with which different tests were used in 72 surveys which were made between 1914 and 1928. Arithmetic and reading were tested most often.

In 1928 about 5,000 college seniors, as well as many thousand seventh grade pupils and high school seniors, were given comprehensive batteries of achievement tests in connection with an extensive project made possible through a coöperative plan worked out by the Carnegie Foundation for the Advancement of Teaching (23), the State Department of Education, and various schools of higher learning in the state of Pennsylvania. Full analysis of the results from the college group has not been completed. A carefully devised plan for following up the younger groups has been put into operation. This college achievement test, which is by far the most extensive test of its kind which has been devised up to date, is not yet available for general distribution. The fifth and sixth annual reports

of the testing programs put on by the Public School Publishing Company have appeared (103, 67). The reports give median scores obtained by grades and by subjects. These programs have been named Annual Nation-Wide Testing Surveys by the publishing company. The latter survey was based upon the following odd combination of tests: The Pressey Diagnostic Reading, the Williams Reading, and the Detroit Mechanical Aptitude Tests. Gerberich and Stoddard (48) report the results of the annual Iowa High School Survey, in which the test batteries making up the University of Iowa entrance examinations are used. Comparisons between average scores for all high school seniors and university freshmen show only slight superiority for university freshmen as a whole, but there is some evidence that the use of the survey results is becoming a factor in encouraging superior students to go to college. Abelson (1) reports the results of the application of the Stanford Achievement Test to 169 sixth grade pupils under the Dalton plan. In terms of accomplishment quotients the bright pupils were 2.3 points above the norm, the average pupils 10.5 above, and the dull pupils 9.3 above.

Henmon (63) reports some results from the Modern Foreign Language Study. Norms for achievement in French are presented graphically for England, Canada, and the United States, the United States excelling in vocabulary, grammar, and silent reading, England excelling in composition. The differences are due in part at least to different arrangements of courses. Much overlapping and large variability were found. Shimberg (124) reports a comparison of urban and rural children of grades 4 to 8 on two different tests of information, one adapted to and standardized on 6,477 city children and the other based on 4,875 country children. On the test standardized on city children the country children fell about a year behind; on the test standardized on country children the city children fell about a year behind. Van Wagenen (143) reports a statewide survey of Minnesota schools which was designed to afford a relative evaluation of eight-month versus nine-month schools, and of small rural versus consolidated schools. An effort was made to test every measurable phase of school achievement. Appreciable superiority was found for the nine-month schools over the eight, and for the consolidated over the small rural schools. Wilson and Ashbaugh (156) conducted a survey to determine whether pupils in consolidated schools showed better achievement than pupils with the same intelligence in one-room schools. In the study four consolidated and fifty one-room rural schools in Ohio were tested on the

Illinois examination. Thirty-three out of thirty-six differences in achievement scores favor the consolidated schools. Pratt (110) compared the standing of the Japanese, Chinese, Hawaiian, and part-Hawaiian children in Hawaii on the Stanford Achievement Test and found, on the basis of averages, that the Chinese made the highest scores and the Hawaiian the lowest, the difference between these two groups being almost a full grade.

Paynter and Blanchard (105) report a study of the educational achievement of 330 problem children in two large cities who were referred to child guidance clinics for reasons other than retardation in school. These experimenters did not find the maladjusted children in the two samplings studied to be distinctly below normal children in educational achievement, but doubtless the factor of selection is a very important one. Different clinics often specialize in different types of cases, and it would seem, therefore, that these findings could be generalized to cover only those samplings where the type of defect studied was the same as in these two clinics.

III. INTENSIVE STUDY OF CURRENT INSTRUMENTS AND TECHNIQUES

In describing the construction of educational tests, Buckingham (18) strongly emphasizes the necessity for including a large number of standardized test items at each level of difficulty. Appreciating the excessive labor entailed by this recommendation, he suggests a short method of standardizing new material by calibrating it against material which has already been standardized. Douglass and Huffaker (38) make an analysis of the negative correlation usually found between I.Q. and A.Q. They conclude that the negative correlation is spurious, due to the fact that the ratio, I.Q., is being correlated against a ratio of which it forms a part, as can be seen from the formula $A.Q. = E.Q. \div I.Q.$ Odell (97) writes a brief elementary account of various measures which have been used to express the relation between educational achievement and native capacity. A large part of the discussion is on the A.Q.

Two studies have appeared which represent attempts to evaluate various methods which may be used in testing spelling. Pintner, Rinsland, and Zubin (108) studied the efficiency of two recognition types of tests as compared with the conventional dictation type. Both types of recognition tests were constructed from words of the Morrison-McCall scale. The reliability coefficient of each recognition form was .84, and the validities of the two forms, as shown by the raw correlations with the Morrison-McCall scale, were .69 and .74,

respectively.* These facts show that the recognition method yields results which are accurate enough for group measurement. Guiler (58) reports a study on practically the same problem. He worked with oral recall, written recall, and recognition. Words from the upper ranges of the Buckingham extension of the Ayres Spelling Scale were used on about 700 students ranging from the seventh grade to college freshmen. His figures indicate, on the basis of averages, that written recall is the most effective in detecting lack of word mastery, though there is considerable variability among individuals, and facts of reliability of differences are not given.

Wilson (154) presents a critical examination of the new Stanford Spelling Test with respect to its usefulness in serving the main curricular aim of the subject, and in reinforcing good methods of teaching. He concludes, from comparison with the Thorndike Word Book and the Commonwealth List, that the test contains many words of unreasonable difficulty and therefore does not satisfy the fundamental criteria of a good test. Wilson and Parsons (155) make a similar analysis of the spelling test used in the so-called Nation-Wide Test of the Public School Publishing Company, and reach quite the same conclusion with respect to it that Wilson reached with respect to the Stanford test.

Henmon (62) presents a wealth of facts about the American Council Alpha Tests in modern languages. This series contains well standardized tests in French, German, Spanish, and an experimental form of a test in Italian. New facts are also given on the Twigg French Vocabulary Test in a section written by Beatley. The data given indicate that the Twigg test correlates more highly with teachers' marks than does the French test of the American Council series. Stoddard (134) has published a description of the origin and use of the Iowa Placement Examinations. Gillespie and Brotemarkle (49) present revised percentile norms, based upon the testing of several hundred college students, for several intelligence tests and a few educational tests, including the Curtis Arithmetic Test and the Trabue Language Scale J. Thornton (140) gives an analysis of the content of ten standard history tests. He concludes that the Hahn and Pressy-Richards tests are superior from the standpoint of dis-

* It is obviously impossible to interpret accurately coefficients of correlation without knowledge of the range in age or grade upon which the correlations were based. However, in the limited space available for this review it is not possible to include more facts than one or two raw correlations on reliability and validity.

tribution of items over historical periods, and the Van Wagenen is superior from the standpoint of relative emphasis upon different fields, such as political, social, and economic. Engelhart (44) gives a summary of useful information on ten standardized tests in the field of education, with a critical evaluation of each. Speer (129) describes an experimental evaluation of seven composition scales: the Hudelson English, the Hudelson Typical Composition, the Leonard, the Lewis, the Nassau County Supplement of the Hillegas, the Van Wagenen, and the Willing scales. Rated in terms of the variability of scores assigned to the same specimens by different judges using the scale, the Nassau Supplement to the Hillegas Scale is the most satisfactory, and the Leonard Scale comes second. Lyman (83) discusses the development of composition scales, and suggests that the trend is towards more analytic types of scales which do not measure the combined product of so many complex factors. Douglas and Lawson (37) present some comparative facts concerning three important reading tests: the Sangren-Woody, the Monroe, and the Haggerty. The Sangren-Woody was the most successful in distinguishing among accelerated, average, and retarded groups in junior high school. These groups had been formed originally on the basis of a combination of teachers' ratings and objective measures of mental ability. Intercorrelations were also computed among these three tests, and they were found to range from .56 to .75. Stabler (130) studied the validity of the Hill tests of civic attitudes and civic information in the case of 120 junior high school pupils. He found a correlation of $-.41$ between scores on the tests and number of civic deficiencies of the pupils as rated by the teachers. The correlation between scores on the Hill tests and intelligence quotients was found to be .27. Markt and Gilliland (87) report some statistical facts obtained in the use of the George Washington Teaching Aptitude Test with 288 education students. Test results were found to correlate with ratings of the girls in practice teaching to the extent of only .12. A reliability coefficient of .64 was obtained. From the correlations it appears that the test measures intelligence as much as anything else. It correlates to the extent of .60 with the Otis Advanced Intelligence Examination, while the correlation between the two duplicate forms is only .64.

The Seashore musical test formed the subject for two investigations. Highsmith (65) studied the results of the Seashore Test of Musical Ability applied to 59 students in a university school of music, and found on the basis of correlations that it measures something

rather reliably, but that this something is not the same thing which is measured in this school by marks in either applied or theoretical music. The reliability of the test was found to be .89. The correlation between test results and marks in applied music was only .31, and between test results and marks in theoretical music, .49. Of course this latter correlation is not very much lower than that between intelligence test results and school marks in the three R's, and therefore the lack of agreement cannot be condemned as being unusual, but the fact remains that in this particular school the Seashore test cannot be used with much assurance for individual prognosis and guidance. Broom (13) investigated the degree of agreement among various measures of students' abilities in music as yielded by the different parts of the Seashore Test of Musical Aptitude. He worked with 102 ninth grade pupils and 82 college juniors and seniors. The agreement among the different tests was found to be significantly greater for the high school group than for the college group, the intercorrelations for the former group ranging from .83 to .96, those for the latter group ranging from .08 to .41.

IV. DEVELOPMENT AND USE OF TESTS FOR IMPROVING MARKS AND MARKING SYSTEMS

The main practical contributions on the topic of improving marks have been in the nature of studies on the best methods of obtaining accurate records of students' abilities by means of examinations. The discussion and study of the so-called new-type examination almost overshadows everything else in this connection. Ruch, in a recent book (116), gives a comprehensive summary of the theoretical considerations and experimental work bearing upon new-type examinations. It gives the most complete account that has so far appeared in one volume concerning the advantages, limitations, construction, and use of each type of examination. One chapter is devoted to relative values of standardized and non-standardized tests, and another to examination marks and marking systems. A small book by Cocks (29) has appeared on the value of true-false examinations. Practical suggestions are given concerning the use of this type of test in physics. Talbott and Ruch (136) discuss the essay and short-answer types of tests as methods of sampling student knowledge, and present an experimental study of the relative efficiency of these two types. The experiment consisted in giving essay-type examinations and very thorough short-answer examinations on the same chapters of various texts being studied by the classes. They

conclude from their results that as a means of sampling factual knowledge the essay type is only about one-fourth as efficient as the short-answer type. Sims (125) studied the reliability of four types of vocabulary test—identification, multiple-response, matching, and checking. He gave by all four methods a list of 70 words selected from the Thorndike Test of Word Knowledge to 110 school children in grades 5 to 8. The reliability coefficients, as determined by the split-half method, are: identification, .92; multiple response, .84; matching, .93; checking, .92. Intercorrelations of the various types range from .54 to .85, the checking test showing up poorest. On the basis of his data, the author feels that, all things considered, the matching test gives the most satisfactory group measure of vocabulary. Jones (74) makes a comparison between a multiple-choice test and a true-false test. He obtains a correlation of .82 between the two types. Edmiston (41) gets some results on a very limited sampling which would lead one to believe that such intercorrelations among the various new-type tests are higher for bright children than for dull ones. Greene and Lane (53) have devised some interesting new-type tests for measurement and instruction in plane geometry. Wrinkle (161), in a discussion of school marks, suggests that objective test results should serve as one measure of objective achievement, and that reports on other factors in which parents are interested should be given separately and not incorporated in the achievement mark.

An interesting extension of this problem of the use of tests in marks and marking systems has been indicated by Taylor (137), who studied the marks or ratings assigned to teachers. By means of partial and multiple correlations he studied the correspondence between ratings of teacher-ability by supervisors and achievement scores obtained by the pupils of these teachers. Each teacher was rated five times. Progress of the pupils in arithmetic and reading comprehension was measured over a period of time. It was found that the agreement between teacher ratings and pupil progress was not large in either subject, but was a little higher in reading than in arithmetic.

V. USE OF TESTS IN PUPIL CLASSIFICATION

One of the most important contributions that has come from the testing movement has been the demonstration of individual differences. One of the earliest uses for which test results were recommended was in connection with the classification of pupils into groups

which would be as much alike as possible in ability to progress. It is interesting to note that the problem of the desirability and success of the use of tests for classification of pupils still affords a lively topic for discussion and experimentation. The experimental results reported this year are no less conflicting than the opinions expressed. None of the experiments conducted this year is conclusive, because each is vulnerable for one or more of the following reasons: (1) The outcomes which are measured are not such as would be agreed upon by competent scholars in the field of philosophy of education as a reasonable sampling of all important outcomes to be considered; (2) variables indicated in (1) which are not in the experimental factor are not satisfactorily controlled; and (3) various parts of the experimental factor are not satisfactorily measured.

Dvorak and Rae (39) report a study of achievement of pupils in segregated and non-segregated groups in the first grade. The segregated group made greater progress in reading, but less in spelling, than the other group. An examination of the teaching methods involved leads the experimenters to suggest that the usefulness of homogeneous grouping depends upon teaching methods adapted to the ability of the pupils. Purdom (113) conducted an experiment covering a period of one semester for the purpose of determining the effect of homogeneous grouping upon progress in English and algebra. Two groups, one relatively homogeneous and the other relatively heterogeneous, were used. These were equated in age and intelligence and taught by the same teachers. The author interprets his results as being unfavorable to homogeneous grouping, but his differences were small and not consistently in favor of either group. McGaughy (85) reviews the advantages and disadvantages of homogeneous grouping and in general feels that the disadvantages outweigh the advantages from the point of view of the well-rounded development of the greater number of pupils. Rock (115) surveys at length current practices in sectioning on the basis of ability. He concludes that actual experimental studies do not at this time show any advantage for ability grouping, on the whole, but he says it must be borne in mind that grouping up to this time has not always been accompanied by the necessary modifications in teaching procedure. Tharp (138) describes an experiment on ability grouping in college classes for foreign language instruction. The sectioning was done by means of the Otis Group Test and the Iowa Placement Examination. The results achieved in the group which was so sectioned, as

compared with the control group, indicate small differences in favor of the grouping method.

Kefauver (77) discusses bases for forming ability groups and concludes that any measure for making such groupings should include (1) a composite of teachers' ratings, (2) intelligence scores, and (3) general achievement test scores. Heilman (60) gives some figures which bear on the relative importance which should be attached to various factors other than educational test results in the formation of homogeneous groups. He reports an experiment designed to investigate the part played by mental age, school attendance, and socio-economic status in determining individual differences in educational age. From study of the records of 828 ten-year-old school children the author concludes that intelligence as measured is eight times as important as either school attendance or socio-economic status in accounting for variability in educational age. Wright (160) reports the success with which standardized achievement tests are used in Indiana for promotion from elementary to secondary schools.

VI. THE USE OF TESTS IN DIAGNOSTIC AND REMEDIAL TEACHING

(a) *Projects Involving the Use of Diagnostic and Remedial Methods.* A number of remedial projects have been reported which involved the preliminary use of tests as a diagnostic measure. Guiler (56) describes an effort to improve the handwriting of seventh and eighth grade pupils. The Gettysburg Edition of the Ayres Handwriting Scale was used to rate the ability of the pupils at the beginning of the experiment. Difficulties were analyzed by means of Gray's standard score card for measuring handwriting, Pressey's chart for diagnosis of illegibilities in handwriting, and West's chart for diagnosis of elements of handwriting. Specific training was given over a period of twelve weeks, during which time it appears that the average quality of the handwriting in the group improved from the third to the sixth grade level. Guiler (55) also describes a remedial project in arithmetic with seventh grade pupils. He concludes that about one full grade's work in certain measurable skills was accomplished in twelve weeks of directed practice. Chase (27) reports an intensive study of the arithmetic difficulties of twelve pupils of a junior high school. A number of tests were used for diagnosis of difficulties, and these were followed by an analytic oral examination of each pupil. Ninety days of individual remedial treatment was given, and at the end of this period much improvement was

found, amounting in some cases to as much as three years. Camp and Allen (22) used a modified form of Gray's Standardized Oral Reading Check Tests in an endeavor to diagnose the difficulties in oral reading of 170 children of grades 2 to 7. Following the diagnosis, remedial work in syllabification and phonetics was given with apparent success. Carroll and Jacobs (25) describe a project in remedial instruction in reading with a group composed of college freshmen. The material used was selections taken from textbooks in different subjects, and arranged in a manner similar to that of the Thorndike-McCall Reading Scale. They report a gain for the drill group of 72 per cent in speed during the experimental period of four weeks. Guiler (57) presents achievement ratings in different subjects for a group of college freshmen and illustrates the general method of remedial instruction which was used in spelling and punctuation. His figures show that large gains were made in these two subjects. Jones (75) describes a project designed to keep the achievement of college students up to their ability levels as diagnosed by aptitude tests. The main measurements used were scores from achievement tests, scores from aptitude tests, and quarterly grades in courses. The efficiency with which a given student was working was measured by a comparison between aptitude scores, as shown by parts of the Iowa Examination, and school achievement. In the judgment of the reviewers, the reliabilities of the two measures which formed the basis of the comparison were not taken into account sufficiently.

Jacobson and Van Dusen (72) report the results obtained from the use of the McCall-Crabbs Standard Test Lessons in Reading in remedial work. These remedial exercises were given once a week to 122 high school freshmen who had been found to be below their grade norm. The average grade score of this group as determined on the Iowa High School Silent Reading Test was 7.1 at the beginning of the experiment. At the end of about six months the same test was given again to the 102 pupils of the group who were still in school, and the average grade score was found to be 9.2. Rhoads (114) also used this well-known series of reading exercises in a study in which he attempted to determine whether wide reading for appreciation or practice in intensive reading is the better method of improving ability to understand words, sentences, and paragraphs. Two seventh grade classes of equal numbers and about equal capacities were used, one for the experimental group and the other for the control. The improvement, as measured by the reading tests of the

Stanford Achievement Battery, was larger for the group which had practiced intensive reading. Maloney and Ruch (86) report an experiment to test the pedagogical value of tests in grammar courses. Four hundred and ninety-seven pupils in the ninth, tenth, and eleventh grades were divided into three groups in each grade, and grammar was taught in the three different divisions by different methods for a period of ten weeks. In one section the textbook method was used, in another section objective instructional tests were used, and in the third both methods were used. Achievement scores at the end of the period showed the test method to be the best and the textbook method to be the poorest. Weinberg (149) has written a brief discussion of the test method in teaching. Washburne (147) presents some interesting material on the value of self-measurement as an aid to learning and recall. He finds that social-science reading material with questions attached is mastered better, by a significant margin, than material with no questions. Moreover, he finds that the best position for questions is at the beginning of a section and that the poorest is at the end.

(b) *New Diagnostic Tests.* The Pressey Diagnostic Reading Test (112) has been designed to measure reading vocabulary in grades 1 to 3. It is available in two forms. Tentative norms are presented. A reliability figure of .92 was obtained on third grade pupils. The same authors have devised diagnostic tests for analysis of reading difficulties in grades 3 to 9. Facts on reliability and tentative grade norms are given. Ingraham and Clark (71) have prepared a diagnostic reading test for primary and intermediate grades. Age and grade norms are given, and some facts on reliability. Bacon's Diagnostic Tests in Latin (4) have been published. These tests are based upon Gray and Jenkins' "Latin for Today: First-Year Course," and are intended as an aid to teaching. No facts on norms or reliability are given. Powers has devised a Diagnostic Latin Test (109) to cover the first year of Latin. Tentative norms are given for students having one and one-half semesters of Latin. Reliabilities of the various parts range above .80. One of the most comprehensive tests of grammar and language abilities that has yet appeared is the new test by Greene and Ballenger entitled the Iowa Elementary Language Test (52). Age and grade norms are given for grades 4 to 9. Reliabilities have been determined for various parts of the test. A Diagnostic Test in Whole Numbers and a Diagnostic Test in Decimals have been published by Brueckner (16). The same author, in collaboration with others, has issued a set of

Diagnostic Tests and Practice Exercises in Arithmetic (14) which they recommend for use with any basic arithmetic text in grades 3 to 8.

(c) *Analysis of Abilities.* There have been several studies which indicate attempts to analyze broad and general abilities into more unitary ones. Such studies and analyses are included under this section because they are sometimes the result of diagnostic testing and sometimes they pave the way for diagnostic testing. Miles and Segel (89) studied the relation of eye movements to reading ability in third grade pupils, and found that a composite eye-movement score correlated .66 with the Gates Silent Reading Test, .75 with the Gates Primary Reading Test, and .70 with teachers' ratings. Symonds and Lee (135) have taken 616 compositions which had been carefully rated on the Hillegas Composition Scale, and analyzed out knowledge of punctuation, capitalization and vocabulary shown at each grade level. Wilson (152) has prepared inventory and diagnostic tests in arithmetic.

(d) *Tests to Measure Units of Instruction.* The Glenn-Obourn Instructional Tests in Physics (50) have been completed after several years of experimental work. The series consists of twenty-five different units which may be used in any order. It is more important as an instructional device than as a measuring instrument. However, tentative norms in percentile units are given. A similar test for use in chemistry has been devised by Glenn and Welton (51). A unit achievement test in plane geometry has been prepared by Lane and Greene (79). Norms and reliability were computed from the results of testing over 6,000 children. Blaisdell (8) has devised an instructional test in biology consisting of twenty-five parts and designed to cover one year's work. Percentile tables are given for aid in interpreting results. Branom (10, 11) has prepared two sets of tests in geography for instructional purposes—one for elementary work and one advanced. These are similar to his earlier series of so-called practice tests. Brueckner (15) has prepared what he calls curriculum tests in arithmetic, covering grades 3 to 8. They are standardized for use at the end of each month.

Instructional tests of the unit type designed to accompany particular text-books are becoming available in greater numbers. The tendency may be illustrated by the diagnostic and measurement tests constructed by Sterling and Cole (133) to accompany "English for Daily Use," and A Standardized French Test, designed by E. B. De Sauzé (36) to cover lessons in "Cours Pratique." Sangren and

Marburger (122) have devised a set of instructional tests in physics consisting of a preliminary diagnostic test, twenty-two instructional tests, and a final examination. The content is based upon five commonly used physics texts. Tentative norms are given.

VII. THE DEVELOPMENT AND USE OF TESTS FOR PROGNOSIS AND GUIDANCE

Brigham (12) submits the fourth annual report of the Commission on Scholastic Aptitude Tests for College Work. The test used and the findings are quite similar to those of the previous year. Facts from typical college groups indicate that scores on the test correlate with standing at the end of the first year in college to the extent of .52. All tests designed to measure mathematical aptitude have been omitted from the examination since 1927. It is interesting to note that the Commission now feels that a supplementary section of the test designed to measure this ability, or group of abilities, would make the test more useful for the selection of scientific students. Condit (31) reports a study of the classification of 559 freshmen entering a teachers' college by means of the Thurstone Examination and the Whitney-Heilman-Woody Entrance Examination. Average scholastic ratings were found to correlate .45 with the Thurstone Examination and .50 with the Whitney-Heilman-Woody Examination. Gerberich and Stoddard (48) report that ratings obtained by high school seniors on the Iowa survey test correlate from .50 to .59 with first semester grades received in college. Longstaff and Porter (81) give a complete list of partial, multiple, and zero order correlations for the following five variables: average of fourteen tests in psychology, final examination in psychology, score on Otis Self-Administering Test, average semester grade in college excluding psychology, and average high school grade. 207 university students, mostly freshmen, were used as subjects. The correlations of the zero order range from .14 to .58. Stanton (131) has written a monograph describing the use of tests of musical prognosis at the Eastman School of Music. Applicants are now classified into five groups on the basis of combined scores on the Seashore Measures of Musical Talent and the Iowa Comprehension Test. A follow-up study of 351 students of the classes of 1925, 1926 and 1927 has, in the minds of the school officials, justified selection on this basis. Sorenson (128) reports a study of the relation between success in various high school subjects and college success. The one high school subject found to be most highly cor-

related with general college success is Latin. French and mathematics were not very far behind. The interpretation of Sorenson's findings is, of course, made very difficult by the uncontrolled factor of the selection of native abilities involved.

A medical aptitude test has been prepared by Moss, Hunter and Hubbard (94). It has been given in about one-third of the class A medical schools of the United States. Statistical data are not yet available. Morris (93) has contributed what appears to be a promising device for predicting success in teaching in elementary and secondary schools. A considerable amount of experimentation has been done, the results of which are described in her thesis (92). The facts so far reported are very encouraging, and it is hoped that further follow-up studies are under way which will help us to evaluate more definitely the practical usefulness of this device. Hunt (68) describes the teaching aptitude test of the George Washington University Series. The reliability obtained by the split-half method from the scores of 100 teachers' college students was .91. Correlations with grades in teacher-training courses range from .50 to .55. Correlations with ratings assigned by supervisory officers to teachers on the job range from .30 to .50. Tentative norms are given. Boardman (9) describes an attempt to devise a set of tests which would predict success in teaching. He includes in his battery an intelligence test, a test of professional information, and a test of teaching procedure. His criterion of teaching efficiency is a combination of ratings from superiors, colleagues, and pupils. The multiple correlation between the criterion and the weighted sum of the tests is .51.

Henmon (64) summarizes contributions made to aptitude testing in the *Modern Language Enquiry*. He reports that the best aptitude test now available in modern languages will not correlate over .65 with grades received in language work. He feels that the best basis at present for prediction of success in a language is a combination of aptitude test scores and scores on achievement tests in that language after a trial period. In this report Brigham presents the facts for the Princeton Artificial Language Test, Hopkins gives the figures for the Wilkins Prognosis Test, and Symonds describes the construction of the Symonds Foreign Language Prognosis Battery. Two important prognosis tests have been prepared by Orleans (101, 100), one for geometry and the other for algebra. In the case of each, validity was measured by correlating scores on the test with later achievement in the subject. For the former

test the correlation was about .80; for the latter the correlations ranged from .71 to .82. Schneck (123) studied the degree to which numerical and verbal abilities are independent of each other. Five tests of verbal ability and four tests of numerical ability were given to 210 college students. The two abilities were found to be largely independent, the correlation between them being only .26. Buckingham (19) reports the results of an interview-test designed to measure the knowledge of numbers of children entering the first grade. The test is concerned with counting, number concepts, and number combinations. From examination of 1,000 children the author concludes that there can be little doubt that first grade pupils are ready for the teaching of number.

Zyve (162) has published an aptitude test for ability to do scientific research. A correlation of .74 between test scores and ratings by judges on a group of fifty graduate science students is reported. Correlations given by him in 1927, from a preliminary study of this test with three different small samplings, were .77, .89, and .95. The test correlates only .51 with the Thorndike Intelligence Examination for a range extending from college freshmen to graduates. The author calls special attention to the difference between the correlation of .74 and the one of .51, concluding from it that the test measures some capacities involved in scientific aptitude other than general intelligence as measured by the Thorndike test. Salisbury and Smith (119) describe the construction of a prognosis test of sight-singing ability. The multiple correlation between an empirical criterion of sight-singing ability and the weighted sum of pitch, dictation and tonal memory was found to be .84. Wallin and Coles (146) present a phonetic spelling scale of 459 words designed to test innate spelling ability. Norms are given in both percentage and standard deviation units. Limp (80) reports statistical facts obtained from a try-out of prognosis tests of shorthand and typewriting which he is constructing. Stedman (132) also presents a study on the prognosis of typewriting ability, but none of the tests which he tried showed encouraging results. Three new tests designed to predict success in shop work have been reported: the Detroit Mechanical Aptitude Examination by Baker and Crockett (6), a shop-work test by Vickers and Hoskins (145) and a test of mechanical ingenuity by Brümmer (17).

VIII. BIBLIOGRAPHIES

One general bibliography, covering research studies in education up to June 30, 1927, has been published by the United States Bureau of Education (142). An anonymous bibliography (2) of 38 titles on educational and mental tests for the blind has appeared. Odell (97) has published an annotated bibliography of 300 titles on examinations and school marks.

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PERSONALITY AND CHARACTER TESTS

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About thirty of the reports appearing in 1929 were based on new tests and techniques. Very little was written about ratings, but the use of controlled observation as a scientific instrument was indicated in eleven studies. There is an increasing tendency to use existing techniques for the solution of problems. That is, the center of interest is gradually shifting from tools to problems. The total number of articles is far smaller than for several years.

Summaries. In addition to the writers' summary (71), Burnham (13), Fisher (27), Furfey (31), Sister McDonough (67), and Watson (115) provide lists of studies.

Behaviors and Traits. Ratings and measures of groups of tendencies are reported by Decroly and Wauthier (22), Henning (47, 48), Jordan (58), McClure (66), South and Clark (95), Steen and Huntington (98), Steere (99), and Vernon (111). Differences between identical twins in sundry traits were studied by Newman (78, 79). Inferiority feelings and emotional instability are dealt with by Ball (3), and Gardner and Pierce (32).

The Colgate inventories, Conklin's device, and the Freyd-Heidbreder list have been used in whole or in part in a variety of experiments by Broom (11), Brownell (12), Campbell (15), Estabrooks and Huntington (26), Garrett (33), Hovey (51), McClatchy (65), South and Clark (95), Weber and Maijgren (116), Wetmore and Estabrooks (117), and Whitman (120, 121). New devices for measuring introversion-extroversion are reported by Newcomb (77), and Neymann and Kohlstedt (80).

Oates (81) continues his studies of the Downey tests, and Harrell and Davis (40) discuss their application to institutional children. They appear also in Newman's studies of twins (78, 79), and Peterson and Lanier's (84) study of negroes and whites, and in a report by Richardson (92).

Pressey's X-O tests, the Woodworth questionnaire and the Kent-Rosanoff association tests were used by Newman (78, 79); Weber and Maijgren (116) included the X-O and association tests in their battery; Dickinson (23) used the Woodworth-Mathews, Wood-

worth-Cady and Kent-Rosanoff tests in a study of stuttering; the X-O was used by Flowers (29), and norms for the Kent-Rosanoff are reported by O'Connor (82). Town (108) reports a new test of emotional balance. Elonen and Woodrow (24) attempted to validate various Kent-Rosanoff measures; Flemming and Flemming (28) studied the validity of the Woodworth-Mathews device; and Gorham and Brotemarkle (36) investigated the validity of the X-O, Brotemarkle, and Downey tests.

Confidence was measured by Jersild (55) and Greene (37); suggestibility by Estabrooks (25), Hull (52), McGeoch (69), and Young (127); social participation by Stoke and Cline (100); optimism and pessimism by Jasper (54); social facilitation by Anderson (2); resourcefulness by O'Rourke (83); persistence by Poul and Montgomery (86); honesty by Bathurst *et al.* (6), Crossland (19), and Tuttle (109); social temperament by Bathurst (5); perseveration by Cushing (20); negativism by Goodenough (35); coöperation, helpfulness, persistence and inhibition by Hartshorne, May and Maller (42); foresight and control by Washburne (113); self-estimation by Schutte (94) and Jackson (53); accuracy and speed by Pollock (85), using the game called *Guidit*.

Interests, Attitudes and Opinions. A technique of measuring interest quantitatively is reported by Wyman (126). Whitley (119) challenges Lehman and Witty's conclusions about the collecting interests of children. Vocational interests were studied by Remmers (88), Lehman and Witty (63), Strong (101, 102), and Strong and MacKenzie (103). Morris' test (76) of the personality of teachers is now available. The effects of various incentives on accuracy of discrimination were measured by Hamilton (39).

Attitude toward monotony was studied by Thompson (104); changes in attitudes effected by a summer camp are reported by Statten (97); sex differences in school attitudes are reported by Witty and Lehman (125); Hersey (50) studied periodic changes in feeling of happiness of male workers. Allport (1) reports a study of political attitudes; Wilkinson (122) investigated differences in social attitudes among several occupational groups. Tuttle (109) and Stabler (96) used the Hill Civic Attitude test; Flügel (30) studied attitudes toward modern clothes. The Purdue rating scale for teachers was used by Remmers (87); Thurstone and Chave bring together in a monograph (106) their work on the measurement of attitude; the attitude of mothers toward sex education is reported by Witmer (123).

Jones (56, 57) has two articles on opinions of right and wrong as held by teachers and children. Miller (72) studied superstitions among college students. Wheeler and Jordan (118) noted the effects of group opinion on individual opinion. Watson (114) reports the opinion of boys about a series of services of worship. Boynton (9) notes the relation of moral judgments to intelligence.

Information and Ability. Religious ideas were measured by Tuttle (109) with a test of Watson's. Moran (75) sought to discover relations between moral judgment and Biblical knowledge. Carmichael (16) reports the knowledge shown by first grade children of conventional responses to imaginary situations common to six-year-olds. Guilford (38) and Landis (61) report studies in the ability to read facial expressions. Garrett (33) used the George Washington Social Intelligence test in his study of race differences. Valentine (110) made a study of intuitive judgment of character by men and women.

Physiological Tests and Physical Type. Chappell (17) reports a study of the effects of deception on blood pressure. Hathaway (43) investigated the relation between association time and the psychogalvanic reflex, using a new psychogalvanic apparatus. Landis and Slight (62) report cardiac responses in emotional reactions. Henry (49) investigated the relation of emotional state to basal metabolism. Rich (9) discusses body acidity in relation to emotion. Mohr and Gundlach (73) report the relation of the Kretschmer type classification to types of crime.

Observation. Hartshorne (41) describes a technique for guiding the observation of social functioning. Bower (7, 8) and Chave (18) report a variety of instruments for gathering facts about conduct and interest.

The behavior patterns of smiling and laughing in infants were studied by Washburn (112). Gesell and Thompson (34) noted the differential effects of training on the emotional behavior of infant twins. Negativism in preschool children was studied by Reynolds (90). Careful record of how 14 four-year-old boys and girls spent their time was kept and reported by Bridges (10). Rugg, Krueger and Sondergaard (93) report a study of traits of kindergarten children as inferred from observed behavior. Civic deficiencies of ninth grade children as observed on 56 occasions was compared with civic information and attitude and school achievement by Stabler (96). Responses of boys to 30 situations and to

incidental occurrences was made the basis of a study of consistency of extrovert-introvert tendencies by Newcomb (77).

Discussion. Basov (4) discusses the developmental course of the processes of behavior. Butterfield (14) raises questions about honesty tests. Dearborn (21) lists tests best adapted to new psychiatric uses. Henning (44, 45, 46) comments on a variety of techniques for measuring character. Kneeland (60) contributes to the evaluation of rating procedures. Liber (64) makes some suggestions for a psychological study of ethics. McDougall (68) discusses the chemical foundations of temperament. McClatchy (65) criticises the concept of social intelligence. Matthews (70) discusses the influence of the position of a multiple choice response on its selection by the subject. Moon (74) reports a study of the reliability of ratings. Reynier (89) notes the need for tests of preference. Thurstone (105) expounds the mathematical principles involved in scaling attitudes. Witty and Lehman (124) continue their controversy with Woodrow on the nature of character tests.

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NOTES AND NEWS

PROFESSOR GORDON W. ALLPORT of Dartmouth College has been appointed Assistant Professor of Psychology at Harvard University and will take up his new duties in the fall of the present year. The appointment is designed to lead to the development of courses and research in social psychology at Harvard.

DR. THEODORE F. KARWOSKI, recently National Research Fellow in the Biological Sciences at Harvard University, has been appointed Assistant Professor of Psychology at Dartmouth College.

The Administrative Board of the Wayne County Training School announces the appointment of Dr. T. G. Hegge as Director of Research. Dr. Hegge has also been appointed Special Lecturer in the Department of Psychology at the University of Michigan.

THE PSYCHOLOGICAL BULLETIN

ATTENTION

BY KARL M. DALLENBACH

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As in the recent summaries of the work on attention,¹ the studies reviewed here will be classified under four headings: (1) studies concerned with the definition and fundamental nature of attention; (2) studies of some special aspect of attention, *e.g.*, range, conditions, fluctuation, levels, measurement, etc.; (3) studies of attention in relation to other phenomena; and (4) technological, or practical, studies.

1. DEFINITION AND FUNDAMENTAL NATURE

(a) *General.* Wolf (36), professor of logic and author of "The Oldest Biography of Spinoza," writes the article on Attention in the 14th edition of the *Encyclopaedia Britannica*. His treatment is wholly inadequate; it betrays no knowledge on his part of the recent work and thought upon the subject. In defining the term he distinguishes between two uses: "in the narrow sense, which coincides with popular use, it means the mental process by which some object comes to be apprehended more clearly than before it was attended to; and in the wider sense, it denotes the mental processes in virtue of which anything becomes an object of consciousness at all as compared with the multitude of stimuli which never find entry into consciousness; although they are within reach of the observer." After illustrating briefly what these definitions mean, he discusses the conditions, kinds, duration, span, and abnormalities of attention. As his treatment of these special aspects is reported below under the proper headings, comment will be reserved until then. We may note here, however,

¹ This BULLETIN, 1926, 23, 1-18; 1928, 25, 493-512.

that his references at the end of the article are limited to James' *Principles*, Pillsbury's *Attention*, Stout's *Manual*, and Ward's article on Psychology in the 1918 edition of the *Encyclopedia Britannica*.

Ford (10) proposes a definition of attention in terms of behavior. He suggests that attention be regarded as the antithesis of automatization, and as the "name for the beginning of a synthesis of reactions to a novel environmental pattern." His definition is not, he feels, antagonistic to the descriptive account of attention, but supplementary to it, because the coördination of reactions—or rather the lack of it—"seems to have certain signs which resemble the conditions described by past workers . . . as attention." He points, in support of this definition, to the results of the experiments on distraction. Distractors at first attract attention—that is, initial reactions under distraction are highly susceptible to interference from the new and hitherto unexperienced stimulus-combination—and the efficiency of the work performed is lowered. With the lapse of time the distractors lose their effectiveness and are not attended to—that is, the reactions are so closely integrated that they are capable of resisting interference—and the efficiency of the work is not affected. Ford obtains further evidence for this thesis from his own experiments upon distraction.

The difficulty with Ford's concept is that his definition of attention is limited to attention multiply determined, *i.e.*, to secondary or voluntary attention. His definition is not applicable to attention singly determined, *i.e.*, to primary or involuntary attention, or to attention habitually determined, *i.e.*, to derived primary or nonvoluntary attention. From a descriptive point of view, there is no difference between these three types or kinds of attention. Ford's attempt to define attention in behavioral terms must, therefore, be marked down as a failure.

Sen Gupta (27), after examining briefly the different theories of attention and finding them all wanting in some respect, advances a theory (which he traces to Sully, Stout, and Shand) that attention is "a process of organic adjustment—peripheral and central—which secures the perseveration of experiences" (p. 163). He bases his theory upon the attribute of duration, and calls it "the perseveration theory of attention." This theory, too, fails to take account of primary and derived primary attention (involuntary and nonvoluntary), besides imperfectly explaining secondary attention (voluntary). While it is an interesting attempt to describe attention in terms other than 'clearness,' yet, since attention does not show an

invariable relation with duration, it was doomed to failure from the outset.

Rodrigue (26) writes of attention as an activity of the will, being closely related to curiosity, and directed by imagination and reason. His exposition harks back to faculty psychology.

Boutan (3), in his principal experiments, attracts the attention of a prosimian (*Lemur macaco L.*). Because the animal showed evidence of fatigue when it had done no active labor, and since certain of the animal's reactions (regarded by many as manifestations of attention) were essentially of a reflex nature, Boutan decides that there is a difference between attention and alertness. He includes the intellectual reactions under the former, and the mass of unintellectual reactions under the latter. Attention is, therefore, for him an intellectual activity.

(b) *Kinds*. According to Wolf (36), attention may be classified either upon the basis of the kind of object attended to (*e.g.*, sensorial and ideational attention), or upon the basis of effort exerted (*e.g.*, spontaneous, voluntary, and involuntary). The more important and less inconsistent principles of classification are ignored.

Dobrinin (9) writes in Russian upon the question of types of attention.²

(c) *Clearness*. The effect of the mental processes by which we attend is, according to Wolf (36), to render the objects apprehended more clear. For Sen Gupta (27), however, clearness is not an essential feature of attentional experience.

(d) *Duration*. Sen Gupta's (27) perseverative theory of attention, discussed above, is based upon the attribute of duration.

2. SPECIAL ASPECTS

(a) *Range*. After an historical review of the traditional 'range of attention' experiment, Glanville and Dallenbach (13) raise the issue whether 'range' is a proper question to set to attention. In one series of tachistoscopic experiments, they endeavored to escape the cognitive complications of the traditional experiment by instructing the *O*s, who had been trained in the observation of attentivity (*i.e.*,

² As the Russian language does not fall within the competency of the reviewer, and as a search in the French and German periodicals did not reveal a review of this or of the other Russian articles listed in the bibliography below, the mere mention of the Russian articles by title must suffice. It is a great pity, however, for many interesting and important facts are doubtlessly thus closed to us.

attributive clearness), to report merely upon the distribution of clearness during the moment of the exposure (*i.e.*, whether it was of one, two, or more levels). To determine how the range of report is affected by degree of cognition, they instructed the *O*s, in other experimental series, to report the number of stimulus-objects apprehended, to name the stimulus-objects apprehended (familiar and unfamiliar), or to describe the stimulus-objects apprehended. In the first series, number of stimulus-objects was found not to be a condition of the attentive distribution of consciousness. In the cognitive series, 'range,' defined as number of stimuli which has a probability of 50 per cent of being correctly reported, varied inversely with degree of cognition and degree of assurance, and directly with kind of material, duration of after-image, facility of verbal association, and color sensitivity. Since, moreover, clearness did not invariably correlate with fullness or accuracy of report, the authors conclude that "questions concerning the number of contents or part-contents that may be simultaneously experienced are questions, since cognition is inherent in report, that concern cognition and not attention."

This study also verified the three conscious patterns first described by Oberly. In the 'immediate' type, the reports are immediately returned. In the 'mediate' type, the reports are mediated by re-imagining or other ancillary processes. The 'immediate' type is divided into two subtypes: the impressions are apprehended either as separate units, or as groups. In accordance with Dallenbach's criticism of Oberly's "transformation" of these patterns,³ they are termed 'unit,' 'group,' and 'mediate' apprehension.

This terminology was accepted by Brown (4) in her study of the effect of homogeneous and heterogeneous chromatic stimuli on the range of 'unit,' 'group,' and 'mediate' apprehension—thus ending a controversy of several years. She cut dots, 6.5 mm. in diameter, from yellow, blue, red, and green Hering papers, and pasted them on white cards. In three series of cards the dots were homogeneous in quality; in four, heterogeneous; and in every series were twelve cards with from 3 to 14 dots. The cards were exposed for 37.5 σ , in haphazard order and by means of a Whipple tachistoscope, 50 times each to two *O*s and 100 times each to three *O*s. The *O*s reported the number of dots seen, the degree of assurance (on a 5-point scale) of their judgments, and the method of determining their reports ('unit,' 'group,' or 'mediate' apprehension). Judg-

³ K. M. Dallenbach. Dr. Oberly on "The Range for Visual Attention, Cognition, and Apprehension." *Amer. J. Psychol.*, 1925, 36, 154-156.

ments with assurance of less than 3 (the certainty of a "1 to 1 bet") omitted. The limens, computed by the method of constant stimuli, are largest for 'mediate' apprehension, intermediate in size for 'group' apprehension, and smallest for 'unit' apprehension; largest, again, for the homogeneous green, blue, and red series, and smallest for the homogeneous yellow series and the heterogeneous series containing yellow. In spite of the anomalous rôle of yellow, Brown concludes that "heterogeneity of chromatic stimuli tends . . . to reduce the magnitude of the limens for each of the three cognitive patterns."

Though studies on 'range of apprehension' do not, as Glanville and Dallenbach's results show, properly belong to the topic of attention, we nevertheless mention them here for historical reasons.

Crosland and Johnson's (6) study on range of apprehension is mistitled. The authors are not interested in the question of range (they do not even compute constants for their *Ss*), but in determining how legibility of printer's type is affected by inter-letter hair-spacing and by the characteristics of individual letters—a topic still further removed from the scope of this review. We must note, however, their use of the technique of the traditional range-of-attention experiment, and their assumption of an invariable relation between report and legibility—an assumption disproved in range-of-attention experiments of the past decade. The conclusions regarding legibility are, therefore, unwarranted inasmuch as they transcend the results.

One result of this study, that "the percentage of letters correct in all respects decreases gradually and consistently from left to right," is favored, as Tinker points out, by the use of letter-series of various lengths (from 3 to 12 letters), "for responses are more uniformly correct with shorter series."⁴ Crosland (5) wrongly interprets this comment as a criticism, as Tinker (30) shows.

Tinker, Roberts, and Jackson (31) study the relative efficiency of apprehension with definite and indefinite preparation. After practice with a definite preparatory signal of 2 sec., they determined the range of apprehension with a definite preparation of 2 sec., and an indefinite preparation of 2, 4, 8, 12, 16, 20, 24, and 32 sec. Computation revealed no reliable differences between spans with definite and indefinite preparation, nor with the various indefinite preparatory intervals.

Wolf's (36) answer to the question of range is "one." "When

⁴ M. A. Tinker. *Visual Apprehension and Perception in Reading*. This BULLETIN, 26, 1929, 227.

objects . . . are exposed for a small fraction of a second, then as a rule only one object is apprehended." If he were writing of the attentive distribution of consciousness, the reviewer would agree with him; but he is not. He is referring to the traditional experiment; and there he is in error, as Hamilton's experiments with marbles, Jevon's with beans, and Cattell's with lines, letters, colors, etc., and numerous later experiments repeatedly demonstrate.

Gundlach, Rothschild, and Young's (14) study may be mentioned because of its use of the method of the 'range of attention' experiment. Ten small electric lamps, arranged at equal intervals around the circumference of an 18-in. circle, were flashed, in numbers from 2 to 9, at different rates. Immediately after the presentation, the Ss (grammar school, high school, and university students) pointed to the lights in the order flashed. Accuracy of response varied with age, practice, and speed of presentation—results comparable to those previously obtained in this field. In their analysis of the functional processes involved, the authors say that they have ignored the traditional concepts of 'attention,' 'apprehension,' and 'memory-span,' because these concepts "too frequently serve to cover up ignorance and to obscure problems." They imagine that "the concept of 'set' has an advantage over these terms." With this point of view the reviewer finds himself in disagreement. The concept of 'set' is in a sorrier plight, as the authors themselves show by giving four very different meanings of it, than any of the "traditional concepts" that they reject.

(b) *Conditions*. Wilcocks (35) undertakes to demonstrate—"that what is heterogeneous, in some respect or other, with the simultaneous or the more immediately foregoing contents of consciousness . . . attracts or holds attention," *i.e.*, that change is a condition of attention; and to test the validity of Selz's laws that attention is attracted by mere difference (*Distinktionsbeachtung*) and by unexpected and opposing difference (*Widerstreitsbeachtung*). To measure the effect of change upon attention by the accuracy of report, the color of one member of the stimulus-series is changed unexpectedly, an old member is replaced by a new member, one member is moved suddenly, or one member is stationary while all the others move. All of these experiments show a "markedly increased memory effect for a heterogeneous content unexpectedly introduced among a group of otherwise homogeneous content," and Wilcocks interprets this increase "as being due to a superior degree of attention having been

given to the heterogeneous content" (p. 280), and as effects occurring in agreement with Selz's laws.

To measure the effect of change upon attention by reaction-time, a loud noise, sounding while *S* is performing simple reactions, is suddenly interrupted, the color of the reaction-stimulus (one of 4 or 12 simultaneously exposed letters) is unexpectedly changed, or the color of the reaction-stimulus (one of 4 simultaneously exposed letters) is the only one not changed.

The cessation of the noise lengthened the reaction-time, corroborating "the influence of an unexpected heterogeneity in drawing attention." The other results, however, are equivocal; when a change is introduced, the reaction is sometimes lengthened, and at other times shortened. To explain these exceptions to Selz's laws, Wilcocks develops a theory "resting, in part, on a hypothesis stated by Henning with regard to an accumulation of energy, in a limited area, consequent on the temporary inhibition of one excitation by another not congruent with it." His results and the theory he bases upon them involve the gratuitous assumption that retention and reaction-times measure attention.

Friedline and Dallenbach (11) investigate distance from the point of visual fixation as a condition of attention. The attentional value of the different distances of two stimulus-objects, alike in every other respect, is determined by the intensity at which the two objects are reported equally attense or clear. The results, calculated by the method of constant stimuli, show that stimulus-objects near the point of fixation have an attentional advantage over objects further removed, and that stimulus-objects to the left of fixation have an advantage over those placed symmetrically to the right of fixation—thus corroborating earlier results that position is also a determinant of attention.

Glanville and Dallenbach (13), in the study reported above, indicate (p. 216) that quality of post-exposure-field, *i.e.*, whether light or dark, conditions attentive distribution.

Wolf (36), following tradition, classifies the conditions of attention as objective and subjective. The objective conditions are intensity, volume, duration, and novelty. The student of attention will be surprised to see novelty in this list until he discovers that Wolf uses that term, not in the accustomed sense of new, strange, or unusual, but in the sense of change. The subjective conditions, which "turn mainly on the momentary preoccupations or the permanent

interests of the individual mind," are idea in mind, attitude, and previous training.

Under this heading belongs also Tassy's (29) short paper on the natural stimulus for attention.

(c) *Fluctuation*. In the face of recent experimental work upon fluctuation of attention, Wolf (36) dogmatically states that we "cannot attend to a single thing for more than about a second without serious risk of falling into a hypnotic trance or similar pathological condition." There is not an iota of evidence for that statement. It is a pity that so much misinformation is given in an article that is supposed to be authoritative.

Dobrinin (8) writes a monograph upon the fluctuation of attention.

(d) *Levels*. In Glanville and Dallenbach's (13) tachistoscopic study of the range of attention reported above, the Os were directed in the first series of experiments to report merely the attentivity of their experience during the moment of exposure. Though the multi-level type of consciousness was tacitly assumed and even suggested in the instructions, it was not reported a single time by any of the Os. Their reports show that their experience during the moment of exposure was divided into two levels of attentivity: a clear focus and an obscure background (p. 213).

Five Russian articles deal with concentrated attention. Birsin (2) writes on the effect of muscle-training upon the capacity of concentrated attention; Bekhterev (1) considers the rôle of concentration in the process of associative-reflex activity; Ilyinski (16) investigates the physiology of concentration; Livshina and Shriftsetzer (17) deal with the group reaction in concentration of children of preschool age; and Oparina (21) reports the effect of disturbances of the functions of the central nervous system upon children's concentration.

Ilyinski (16), according to Shnirman's review,⁵ uses a variation of Bekhterev's reflex method, and shows that concentration should not be regarded as a separate brain process, but as a differentiating activity of the perceptive part of the associative-reflex arc.

(e) *Measurement*. Wilcocks (35), in the experiment reported above, makes no attempt to justify his use of reaction-time as a measure of attention (probably because it cannot be done), but he does attempt to justify his use of memory. "Since, other things being equal, the retention of, and the ability to reproduce, a content varies directly with (not, necessarily, proportionately to) the degree

⁵ A. L. Shnirman. *Psychol. Abstracts*, 1928, 2, 203 (No. 864).

of attention given the content at or after presentation, we are able to make use of the memory effect as a measure of the degree of attention which the content has had." But other things are not equal. As experiments have repeatedly shown—several of which are mentioned in this review—attention and report are not co-variables; clearness of content is only one of a number of factors conditioning report.

Philip (22) reviews the work that has been done upon the measurement of attention, and reports the results of his own efforts to devise a battery of tests that will measure attention. The battery was developed by experimenting upon 49 men, all senior college students, and was standardized by testing 1,600 school children from ten to nineteen years old.

Ford (10) proposes motor tonus as an index of degree of attention (p. 32).

(f) *Distraction and distribution.* Meisenheimer (19) inquires whether "central factors, *i.e.*, associations, always attach themselves to the focus of experience" (*Zentrum des Erlebens*). He sought to test this proposition experimentally by presenting stimulus-objects in peripheral vision, and noting how the Os' descriptions differed when attention was directed to, and away from, the objects. He chose peripheral vision because object-meanings develop slowly there, a favorable opportunity being thus offered for studying the development of meaning and of the conditions underlying it.

The fixation-point was placed 0° , 3° , 6° , or 12° to the right of the exposure-window and at distances of 31.4, 94.2, and 188.4 cm. from *O*. Typewritten digits and Landolt broken rings were exposed—but the results with the rings are not reported because of lack of space. The experiments were performed in a dark room, and the exposure-field was illuminated by a light which was controlled by an electrical key at *O*'s desk.

In Part Ia, *O* was directed to keep his eyes upon the fixation-point, but to attend, as a teacher does to a pupil, out of the corner of her eye, to the exposure-window, and to report immediately after the exposure (the length of which *O* was allowed himself to determine) the course of his experience. An analysis of the reports revealed seven steps or levels of cognition.

Slight changes were made both in apparatus and in procedure in Part Ib. A Hipp chronoscope was attached to the electrical key so that the time of illumination could be measured; only the shortest fixation-distance was used. In addition to the instructions of Part Ia, *O* was told to release the key as soon as he perceived a definite object

in the exposure window. The time of the first definite impression varied greatly for the different *O*s, but it increased for all with size of exposure-angle.

In Part II, *O*'s attention was directed away from the exposure-window to some other designated part of the exposure-field. Digits were exposed in both places, and the *O*s, most of whom were trained in introspection, gave complete reports of their experience. An analysis of these reports revealed five descriptive levels: one lower than the lowest found in Part I, and the other four agreeing with the first, second, third, and seventh steps of Part I.

The results reveal two facts which Meisenheimer believes to be contrary to all the existing theories of attention: (1) attention is primarily the experience of a turning-impulsively-upon, with which the phenomenological focus co-varies; and (2) all phenomena appear for the first time in the focus of experience and are developed only there; but after their first appearance they may be continued outside the focus of experience in a wider conscious field and there remain, where attention may again be turned upon them.

The exposition is needlessly complicated by the use of numerous arbitrary abbreviations. The provincial nature of the study is shown by the fact that every one of the 45 references in the bibliography is German.

After an historical and critical survey (far from complete) of the experiments upon distraction, Ford (10) reports experiments of his own. His distractors are a phonograph and an automobile horn; his measures of the effect of distraction are: (1) efficiency of work, measured by time and accuracy of adding a series of single-digit numbers scattered haphazardly in a row of mixed letters, (2) writing-pressure, (3) respiratory changes, and (4) vasomotor changes.

A series of 6-8 control experiments precedes and follows a series of 6 distraction experiments. The 6 experiments immediately preceding and immediately following the distraction series are used in computing the results. There are 17 *S*s (Ford says 16, but his data show the results of 17) in the experiments with phonographic distraction, and 41 *S*s in those with the automobile horn. The data for every position within both the control and the distraction series are averaged separately and curves are plotted. The results are: (1) initial addition takes longer than any later addition having constant environmental stimulus-patterns; (2) there is a slight tendency for more errors to be committed in the first half of a given homogeneous period than in the second half; (3) writing-pressure

and addition-time show a reliable positive correlation of 0.55; (4) vasomotor concomitants show initial effects, which go through gradient changes as the same type of work continues under constant environmental stimuli; and (5) fast workers quickly become automatized, slow workers only gradually. Two of Ford's diagrams (p. 24) appear to the reviewer to be mistitled. The diagram for the "fast workers" shows a slower addition-time than the diagram for the "slow workers."

One of Wilcocks's (35) experiments in the study reported above is concerned with the effect of distraction upon attention. He uses the simple visual reaction-time as a measure of the effect, and the cessation of a concomitant auditory stimulus as the distraction. A series of 10 reactions was performed while a comparatively loud noise sounded close to *S*. Just before the stimulus was given for the eleventh reaction, the noise was suddenly interrupted. The numerical results and the introspective reports both show that "attention was attracted to the sudden cessation of the noise."

An adaptation of Piorkowski's attention-fatigue meter is described by Verwoerd (32), who believes that it successfully measures the distribution of attention. He did not, however, find (33) the apparatus suitable for determining the effect of fatigue under disturbed attention. These studies are reported fully below.

Hovey (15) in a study of the effects of general distraction on records made in the Army Alpha Intelligence Test, concludes: (1) higher mental processes are comparatively unimpeded by distraction; (2) intelligence is not related to susceptibility to distraction; (3) there are no individual differences in susceptibility to distraction; and (4) true mental ability is more nearly approximated under distraction than under standard conditions.

Skaggs (28) shows, from a study of Hovey's data, that the distractors have a detrimental effect when practice is taken into account, and that Hovey's conclusions are not warranted.

Miles and Terman (20) review and comment upon the distraction experiments in word-association (pp. 174-179) that reveal sex differences.

Mandeville (18) discusses the causes of distraction, and gives practical suggestions for overcoming it.

(g) *Abnormalities*. Abnormal forms of attention, according to Wolf (36), are scattered attention and fixed ideas. The abnormalities are, in the opinion of the reviewer, more a matter of association than attention.

3. RELATION TO OTHER PHENOMENA

(a) *Intensity*. Wolf (36) states that clearness and intensity are different and, to a certain extent, independently variable aspects in the apprehension of objects.

(b) *Attention and association*. Wheeler (34) traces the development of the concepts of attention and association from the epistemological theories of the Continental rationalists and the British empiricists to the present time. He holds that these concepts have been used from these different points of view to account for the same set of facts, *i.e.*, "the orderly grouping and seriation of detail in mental life" (p. 2). The two schools faced the same logical problems, and they reached different solutions because "each allowed its epistemological interests to dictate its psychological system-making" (p. 5).

In spite of the faults of the apperceptionists and the associationists, the problem they faced is as alive today as at the time they were first confronted with it. If a systematic psychology is to be built, it cannot thrust either solution aside, for the logic which compelled them "to search for a common denominator in terms of which to envisage continuity and unity in complex mental life is as vital and legitimate today as it was then" (p. 6). That is Wheeler's thesis, and he devotes the rest of the paper to defending it. His conclusion is that "association is the problem of pattern and attention is the problem of the form and limits of pattern" (p. 17).

The exposition is marred by inaccuracies and by many misstatements of fact and theory. For example, Wheeler's statement that "Wundt discarded the two-level notion of conscious and subconscious and substituted for it the notion of focus and margin" (5) is not only inaccurate, but betrays a complete misunderstanding of Wundt. Focus (*Blickpunkt*) and margin (*Blickfeld*) are merely metaphors. Wundt used them, as Fortlage, Lotze, and Tucker did before him, to illustrate the difference between the perceived and apperceived levels of consciousness—a difference first pointed out by Leibnitz. Wheeler's difficulty with 'fringe' and 'margin' is entirely of his own making. 'Fringe' does not have to become 'focus' in order to be observed. Again, Wheeler misunderstands and misstates Titchener. Titchener did not regard attention as a process, nor 'attensity' as a dimension of consciousness. Wheeler categorically denies the distinction between attributive and cognitive clearness, but does not give the evidence upon which he based this judgment. His statement that the "distinction is untenable" is sheer dogmatism.

(c) *Attention, concept, and judgment.* Prengowski (25) considers attention, concept, and judgment together, because he finds close relationships among them. His treatment of the three phenomena is logical rather than experimental, and philosophical rather than scientific. The study is of no great interest to the student of attention.

(d) *Motor phenomena.* Ponzo (24) shows rather conclusively that the respiratory changes accompanying sensory attention are due, not to attention, but to the aim or purpose of the sensory act. The changes, for example, accompanying attention to olfactory stimuli are very different from those accompanying attention to weak sounds. If attention were responsible for the changes, they would be the same in the two cases. The inspiration-expiration ratio decreases, he finds, in all kinds of mental activity, and is best explained, he believes, as a facilitative modification.

Ford (10) measures the effect of distraction upon attention by way of motor, respiratory, and vasomotor changes. He finds a positive correlation of 0.55 between the pressure used by his Ss in writing the sums of the problems in addition and the time taken by them to make the additions. Since he regards a long addition-time as an indication of the lack of automatization and therefore of the presence of attention, he concludes that "we must abandon any theory that continuous and rapid work under noise conditions will be accomplished at a sacrifice of muscular energy" (p. 37). His pneumographic records show so many inhibitions of breathing that he finds it impossible to draw conclusions from them. The vasomotor records, obtained from a finger plethysmograph, show "that any change of mental processes, whether we call them attention, emotion, feeling, or what not, produces an initial constriction followed by gradual dilation" (p. 30).

4. TECHNOLOGICAL

(a) *Attention tests.* Verwoerd (32, 33) reports the results of two experiments with Piorkowski's attention-fatigue meter—an apparatus which is little known in America, but is in principle nothing more than a multiple-reaction apparatus. It consists of a rectangular box in the top of which there is a row of 10 small windows paralleled by a row of 10 reaction-buttons. White squares are pasted upon a black drum within the box in such a way that, when the drum is rotated, they will appear, in the various windows, one at a time and in irregular and haphazard order. The person being tested is directed

to react as soon as he sees the white stimulus-object by pressing the button opposite the window in which it appears. The number of stimulus-exposures and of correct reactions is automatically recorded by electrical counters. This apparatus, we are told, is widely used in Germany for testing attention.

Three assumptions, according to Verwoerd (32), underlie this use of the apparatus: (1) the distribution of attention is responsible for performance; (2) the reactions, in spite of their complexity, do not influence performance; and (3) either the correct reactions are evenly spread over the 10 buttons, or there is a high correlation between performance, as measured by the total number of correct reactions, and the distribution of attention. His problem was to test these assumptions. He required his *Os*, 25 in number, to report at the conclusion of the experiments the manner in which they performed the task. These reports, supplemented by *E*'s observations, were used as checks of the first two assumptions. The third assumption was checked by attaching electrical counters to every one of the reaction-buttons, thereby making it possible to measure performance separately at every position.

According to his results none of the assumptions is justified. The reactions soon became automatized and the *Os* performed the task in a state of inattention as efficiently as in the state of attention, or even more efficiently. The apparatus cannot, therefore, be used in the prescribed manner to test attention. It is at most merely a test of the distribution of marginal consciousness, and of automatic action. The distribution of the scores among the 10 positions revealed an 'even' and an 'uneven' type of performance. Since the performance of these two types was found to be incommensurate, a single basis of ranking the *Os* could not be obtained.

In an endeavor to utilize the apparatus for testing attention, he altered the method. He instructed the *Os* to react on the fourth button with the left hand when a stimulus-object appeared in any one of the 5 windows to the left, and on the seventh button with the right hand when the stimulus-object appeared in any one of the 5 windows to the right. Since the *Os* did not under these conditions become habituated or react automatically, he concludes that the apparatus is serviceable when used, by his modified method, for testing the distribution of attention.

The distribution of performance among the 10 positions (how this was obtained is not clear, as the *Os* reacted only on the fourth and seventh buttons) varied as before. 'Even' and 'uneven' types

of performance were found. Consequently, a single basis of evaluating performance could again not be given.

Using this modified procedure, Verwoerd (33) undertook to discover the effects of fatigue upon the distribution of attention. Various effects were distinguished: (1) the performance as a whole suffers; (2) the unevenness of distribution increases, though the general level of performance remains the same; and (3) the distribution is narrowed, though the general level again remains unchanged. On the whole, however, Verwoerd finds that Piorowski's apparatus "is not suitable for testing fatiguability where sustained distribution of attention is necessary, even if the apparatus is so altered that a distribution of attention is really obtained and that the evenness of such distribution can be checked."

Mme. Piéron (23) standardized Toulouse and Piéron's modification of Bourdon's letter-cancellation test. In the modified form two symbols are cancelled in a list of 1,600, composed of eight different kinds. The test serves as the attention-test in a battery designed for professional orientation. Norms were established for 1,091 Paris school children (489 girls and 602 boys).

Gamsa and Salkind (12) compare and evaluate the results obtained from (1) Rybakoff's color-counting test, (2) Rybakoff's square-and-cross counting test, (3) Schultze's number-square test, (4) Toulouse and Piéron's symbol-cancellation test, and (5) Rosolimo's distribution-of-attention test. These five tests were given to 90 girls (nine, twelve, and fifteen years of age) and to 50 women. Quartile records are furnished for every test, and inter-correlations for the adult scores are computed between every two tests. The relative gains vary considerably among the different ages and among the different tests. The correlations among the tests, though all positive, are slight, varying between 0.09 and 0.37.

Philip's (22) battery of attention-tests, devised especially for use in the schoolroom, includes variations of the familiar tests of number-span, mental multiplication, mental addition, alphabet, and cancellation. The battery was given to 1,600 school children, from ten to nineteen years of age, and performance in every test was separately computed for every age. The curves of performance in most of the tests reach a plateau at the age of fifteen. A correlation of 0.09 was obtained, from a sample of 243 children, between mental score and 'attention score' as measured by this battery.

(b) *Hygiene of attention.* De Lambert (7) writes on the

hygiene of attention, in the belief that attention is an activity that can be controlled through conscious self-regulation.

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MEMORY¹

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I. MEMORIZING

The Curve of Memorizing. The form of the learning curve for motor skills, and the factors affecting it, have long been among the most active problems of the acquisition of skill. Upon the corresponding problems of memory,³ however, comparatively little work has been done, although it is theoretically as important to know the temporal course of acquisition in one field as in the other. The reasons for the disproportionate emphasis upon the curve of skill to the neglect of that of memorizing are probably intrinsic to the techniques and materials of the two fields. The period of acquisition of most of the materials used in experiments upon memory is, for example, shorter than is that of most skills, and the plotting of a curve is a less obvious procedure. Further, a majority of the experiments upon memory are conducted by methods, now traditional, which do not yield a measure of progress at the end of each repetition, thus obviating the possibility of curve plotting. While these factors may account for the neglect of the curve of memorizing, they are scarcely more than historical accidents and certainly constitute no actual barrier to its study.

During the period covered by this review, but three titles refer with any directness to the curve of memorization. In an experiment by Foster (48) eight simple stories were read to pre-school children individually. At the first presentation each story was read through continuously. On nine succeeding readings the experimenter paused at certain definite places, about forty in each story, and waited for the subject to supply the words. While not all stories were given

¹ This review covers the period since the preceding review by the writer, "Memory," *Psychol. Bull.*, 1928, 25, 513-549, with the addition of a few titles for 1927. It includes experimental and theoretical papers, general treatments of memory, and reports of new apparatus and problems. Not all titles are mentioned in the body of the review.

² Research paper No. 177, Journal Series, University of Arkansas.

³ The traditional distinction between the provinces of skill and memory is assumed for the purposes of this review.

under exactly the same conditions, no warping of results will occur by reporting composite curves. When words per child per story are plotted against repetitions, the curve for the group with a mean CA of 2-11 (years and months) shows a clear initial positive acceleration, those for age groups 3-4 and 3-8 present less positive acceleration, while that for age group 4-3 is almost linear. Likewise when the subjects are divided into groups with mean MAs of 3-2, 3-10, 4-6 and 5-8, the curves for the two lower groups are positively accelerated (that for the lowest MA group rises very slowly in a straight line, but it is fair to assume that it will turn sharply upward somewhere beyond the number of repetitions used, since the other curves show such a turn, though earlier), and those for the two higher are either linear or very little positively accelerated. The curves for the harder stories show more initial positive acceleration than do those for the easier. Markey (113) has reproduced several curves, for the acquisition by children of personal pronominal symbols, which show a pronounced initial positive acceleration, a characteristic which also appears in the scores of Martin and Fernberger (114) for improvement in memory span. The latter fact is particularly significant since only the best record of each day is included.

It seems likely that with memorial materials, as with motor skills, very young subjects, hard problems, and relatively low intellectual level are factors which produce a curve with an initial concavity to the y axis.

The Relation Between Length of Series and Difficulty. Thurstone (168) has tested, on well known data, two hypotheses regarding length of list and difficulty. According to Hypothesis I the relation between length and time should follow approximately the following

law: $T = \frac{c}{k} n \sqrt{n-a}$, in which T is the total learning time, k is

the learning constant of the subject, c is the criterion of learning (a constant), n is the number of items in the list, and a is the attention span of the subject. Since a virtually disappears in significance for long lists, the hypothesis states that learning time varies approximately as the $3/2$ power of the length of the list. The equation given above has been fitted to some data from Binet and Henri, and to four sets of data from Lyon, and a very close agreement has been found between the theoretical curve and the experimental data.

According to Thurstone's Hypothesis II, the relation between the

number of repetitions and the number of items in the list should follow approximately the law, $R = \frac{c}{k} \sqrt[n-a]{\quad}$, in which R is the

number of repetitions and the remaining notation as in Hypothesis I. This equation has been fitted to the classical data of Ebbinghaus and of Meumann, and to data from Lyon. The theoretical curve fits well the data of Ebbinghaus for the longer lists, but only approximately those for lists of 7 and 12 syllables. It fits poorly Meumann's data, but with Lyon's the agreement between the theoretical equation and the experimental results is remarkably close.

Both hypotheses are limited to meaningless material learned by a single subject who has reached, before the experimental records are taken, a practice level for the material and method employed. Under these conditions the hypotheses that, save for short lists, learning time varies as the $3/2$ power of the number of items, while number of repetitions varies as the square root of the number of items, yield a gratifyingly close agreement with experimental facts.

Skaggs (156) has suggested that the apparently discrepant conclusions of Ebbinghaus and of Meumann are both true within certain limits, and that the true relation between length and number of repetitions is an S-shaped curve. There is no contradiction between this suggestion and Thurstone's theoretical relationship, since the latter's equation takes account of very short lists by correcting for attention span. He also considers it probable that agreement with short lists near the span is not to be expected. In the curves obtained by Vogelsang (173) for the relation between number of items and the exposures required to learn them in varying arrangements, wide individual differences occur.

It seems to the writer that the results of Bills and Brown (9) upon quantitative set, while done entirely upon a problem in the field of work, have implications which cut across several of the problems of learning. It can hardly be doubted that memorization involves both work and acquisition, and that the latter masks any decrement which may be present. Bills and Brown find that the decrement varies directly as the amount of work with which the subject is faced at the start, *i.e.*, with his quantitative, 'amount' set. In learning it may well be expected that a subject's 'amount' set will correspond to the length of the list which he has before him to learn, and hence the longer the list the greater the relative decrement will be. Thus with long lists the decrement may operate as one of several factors to increase disproportionately the time required to learn, and with

short lists its lessened amount may weigh in producing the rapid learning thereof. It may be, as well, a factor to be considered in the part-whole issue upon which no paper has been published during the period of this review.

Distribution of Practice. One experiment dealing primarily with this well-worn problem has been published, together with several incidental confirmations of previously reported conclusions, and one review. Spight (160) has made an important comparison of the effect of two 12-hour rest periods introduced at the same stage in learning, one during the day and the other during the night, upon the learning of lists of 14 paired words. During the first sitting the material was presented until the subject was able to recall at least half of the items, and 12 hours later the learning was completed. The groups were equated in age, training and memorial ability, and the order in which the lists were learned was counterbalanced. The trials for the first half of the learning were almost identical for the two groups, but completion of the learning after 12 hours of which the major part was spent in sleep was significantly superior. Conditions were such that the difference must be a function of the filling of the interval. It is concluded that the greater ease of learning after the night interval is probably the result of physiological facilitation, since the memorial review which is at least possible during the day interval is ruled out. There is a second possibility, *viz.*, that during the night interval interpolated activities were minimal, with a resultant forgetting from retroactive inhibition which was less than that produced by the numerous interpolated activities of the day interval.

The general efficacy of distribution is confirmed by Jersild (90), who finds that two repetitions are more effective when an interval occurs between them than when they are presented in immediate succession; by Boring's (14) result that equal distribution of 12 repetitions over 3 days is better than 12 massed repetitions; and by Foster (48) who reports that 5 repetitions per day for 2 days are not as efficacious for the learning of stories by pre-school children as is distribution to the extent of one repetition per day. Vélinsky (171) finds that, where the intervals between successive presentations are not large, an increase in interval is accompanied by an increase in the number of presentations required for learning. Certain limitations upon distribution are implied by Ghosal's (54) conclusion that postponing fixation after meaning has been set up may be detrimental because of the concurrent weakening of meaning.

Ruch (147) has reviewed the literature on the optimal distribution of work and rest periods in relation to the various other factors affecting learning. His review includes studies of both memory and skill.

Age Differences. Now that the general increase in memorizing ability with increasing age has been established beyond cavil, research interests are being extended to include the course of the curve beyond the years of late adolescence. The highly significant work of Thorndike and his associates (167)⁴ has shown that persons between the ages of twenty-five and forty-five are distinctly superior to children in learning ability and equal or superior to persons between fourteen and eighteen. The decline from the early twenties to the late forties is not more than from 13 to 15 per cent. The subjects were tested on representative materials, including a logical system (Esperanto) and school subjects. The decrease in learning which, on the average, is found to occur in the later ages may be ascribed to various combinations of the four factors, health and energy, opportunity, interest or motivation, and ability to learn.

The age curve for the recall of the symbols of the Army Beta digit-symbol test, used in Willoughby's (179) study of incidental learning, has a mode in the vicinity of age twenty-two and drops away sharply toward childhood and gradually toward old age. Jones, Conrad, and Horn (95) have constructed a composite curve for tests of the recall of the verbal and pictorial contents of motion pictures by persons, varying in age from ten to sixty, from 8 Vermont villages. Care was taken to render the form of the curve free from the influence of special selection within the communities involved, and from other extraneous factors such as visual acuity and interest in the pictures. Increase with age is virtually rectilinear to age fifteen, with negative acceleration from that point to age twenty-three. From forty-five to sixty there is a rectilinear decrease, and by age fifty-five performance has fallen below that for age thirteen.

Five studies corroborate the traditional results for the early years and one fails to do so. In Foster's (48) work on verbal memory for stories, the group of children with a mean CA of 4-3 start at a higher point and improve faster than the younger children; ages 3-4 and 3-8 are close together in performance; while the subjects of age 2-11 are clearly inferior to those at the higher ages. The older and more mature children are, however, more likely

⁴ Reviewed at greater length in McGeoch, J. A., *The Acquisition of Skill*, *Psychol. Bull.*, 1929, 26, 457-498.

to remember the meaning of a passage and more apt to stray from exact verbatim reproduction. The correlation between CA and total score for verbal memory is 0.74 ($N=22$). Barlow (3) has published curves which show a steady increase in number of syllables learned from Grade II to the college years, and McGeoch (119) finds that ability to report correctly in both the narrative and interrogatory forms increases slowly from age nine to age fourteen. Differences between successive age levels are small and are statistically unreliable, but they accumulate in such a fashion as to render the differences between the extremes of the age-range much larger and of a satisfactory degree of reliability. Narrative errors have no definite tendency to vary with age. Interrogatory errors decrease very slightly with increasing age.

According to Fischler and Ullert (46) memory span for digits and memory for geometrical figures increase very slowly, if at all, from age seven to adulthood; memory for words increases irregularly; and memory for pictures and for phrases show a clear age increase. The authors interpret the latter fact to mean that, with these materials, logical organization can enter, as opposed to the brute memory involved in digit-span and recognition of geometrical figures. They question the inclusion of the digit-span tests in the Binet-Simon scale, since the span presents so little clear increase with age. In all 530 subjects were employed and it is difficult to understand this divergence from the usual result unless the factor of interval operates against the influence of increasing age, which seems unlikely. Fischler and Ullert used a rate of two seconds per digit, which is much slower than the Stanford-Revision rate of slightly more than one digit per second. Young (181) obtains some increase with age in both forward and reverse spans. Learning span increases more definitely with age. Netschajeff (130) finds a steady and considerable increase in both digit-span and sentence-span between ages four and eight. In Garrett's (53) experiment, logical memory is the only test not negatively related to age in a group of 158 college men.

Sex Differences. The once eager search for fundamental intellectual differences between the sexes has noticeably abated, but conclusions regarding the relation between sex and mental functions continue to be reported, although they are for the most part incidental to other research interests. In the learning of lists of 20 syllables each, with full and restricted articulation, the girls excel the boys (Barlow, 3). The differences are not wholly reliable, but there is a probability in favor of feminine superiority. The total number

of cases is large. Ridgley (144) reports that, in an experiment aimed at studying children's interest in learning about places, the girls exceed the boys by 10.7 per cent in terms of the places actually looked up. McGeoch (119) has studied the problem of sex differences in testimony or report. Sex differences are found to be, in part, a function of the material upon which report is made. Reports upon the Binet object-card show no consistent sex differentiation, while reports on a picture, and even more on an event, show a somewhat consistent, though slight, tendency for the girls to excel. The differences are small and few are reliable. Assurance, measured by the per cent of items attested, does not vary regularly with sex. In recognition memory the women among Seward's (155) subjects have higher confidence ratings than the men. Recognition time has a greater range with the men; and there are no reliable differences between the sexes in mean speed or in relative variability of recognition times. In verbal memory for stories, the boys of Foster's (48) experiment consistently excel the girls, but the number of cases is small. Washburne's (174) experiment upon the use of questions in the learning of social science material reveals that girls are somewhat more affected by preview questions than are the boys, while with review questions the reverse is true.

Three other investigations yield no sex differences. The five immediate memory materials employed by Fischler and Ullert (46); the recall of incidentally learned symbols (Willoughby, 179); and learning both normally and under the influence of distraction from concurrent stimulation (Schorn, 149) fail to present such differences.

In studies on this problem one never knows whether the differences which appear, even granting their statistical validity, are inherent in the subjects or whether they are functions of the subjects' previous acquaintance with the material, and of other adventitious factors. Moreover, the reported differences uniformly fail of statistical reliability. It may safely be concluded that sex differences in memory, of whatever nature they may be, are very slight, probably inconsequentially so.

Intelligence and Memorizing. The far reaching implications, both theoretical and practical, of the problem of the relation between intelligence and memorial ability conspire to render it an increasingly active problem. Garrett (53) has conducted an extensive experiment, using 158 college men as subjects, in which he has correlated five tests of memory: visual and auditory digit-spans, visual and auditory paired-associates, and logical memory for fairly difficult prose; with

intelligence (Thorndike examination). The multiple correlation of intelligence with the five memory tests is 0.44, which becomes 0.52 when corrected for attenuation. The addition of three learning tests of the order of skill raises the corrected coefficient to 0.60. Individually the memory tests yield low correlations with intelligence. The multiple correlation coefficients are of no small significance because of the highly selected character of the group. The discovery of a substantial relationship between intelligence and memory records in comparatively routine tests, a relationship which would doubtless mount with the addition of more tests and finer measurements, points, Garrett thinks, to the essential oneness of intellect, a unity in terms of the Thorndikian quantity hypothesis, whereby simple connection-forming and high sagacity are basically the same. McGeoch (121) finds that the correlations of combined sigma standings on NIT and Army Alpha with correct narrative and interrogatory reports on the Binet object-card, a picture and an event are positive at every age from nine to fourteen and that most are high enough to indicate a definite relationship. With the two materials first mentioned, the interrogatory yields a higher correlation with intelligence than does the narrative. The height of the coefficient shows some tendency, also, to vary with the material upon which report is made. Total narrative errors are unrelated to intelligence, while total interrogatory errors are related negatively. Per cent of attestation is wholly unrelated to intelligence. In Hartmann's (71) study of precision and accuracy there appears a correlation of 0.05 between the Thorndike Intelligence Examination and interrogatory reports on pictures. Cushing's (36) conclusion that perseveration depends more upon mental age than upon chronological maturity has important implications for memory problems.

The relation between MA and the form of the curve of memorization for words in stories has already been discussed (Foster, 48). When pre-school children are divided into groups with mean MAs of 3-2, 3-10, 4-6, and 5-8, the curves for the acquisition of words by each group run at a level corresponding to the MAs of the group. A fairly high correlation appears (Hegge, 73) between mental level and answers to questions upon a simple story by a group of subnormal children. Above an MA of 5.3 no total failures occurred. Farson (44) obtains evidence of a positive relation between memory span performance and school success, and Willoughby (179) reports a correlation of 0.686 between school grade and the recall of incidentally learned symbols. Such recall correlated but 0.133 with

brightness. Anderson and Jordan (1) present a correlation of 0.65 between I.Q. and immediate memory, and Lowery (109) has published a coefficient of 0.44 between the Tomlinson 'West Riding' tests of mental ability and musical memory. Schorn (149) finds a rough positive relation between immediate memory for an event, enacted while the subjects worked at arithmetic sheets, and scholastic ability. Intelligence seems to be a differentiating factor with respect to the optimum method of acquiring academic material (Greene, 62). Students whose Thorndike Intelligence Examination scores, or whose college grades, fall in the upper quartile of the subjects used, tend to learn slightly better by reading the material for themselves than from lectures. The opposite is true for the lowest quarter of the students. Independent learning (Herrmann, 76) corresponds somewhat more highly with estimated intelligence than does ordinary school learning.

It will have been noted that the relationships between intelligence and memory which have been summarized, in common with those previously reviewed, are uniformly positive and consistently low. The substantial multiple correlation obtained by Garrett between five memory tests and one of the best existing measures of intellect would, however, lead one to suspect that the small relationship between single measures of memorial function and single intelligence tests are but the outcome of numerous factors specific to the experimental conditions, and that a composite of a very large number of variously obtained records of memorial ability would approach as a limit that which is measured by tests of general mental ability.

Sensory Modality and Method of Presentation. Sensory modality and the methods of presenting learning materials are variables which are closely interrelated, and the experiments upon the two will be reviewed together. Tilton and Knowlton (169) have made a well-controlled attempt to measure the contribution of ten chronicles-of-America photoplays to history teaching in the seventh grade. Under the influence of the photoplays the experimental group gained 19 per cent more than the control. To show in another way the significance of this gain, a control group was selected of sufficiently greater ability to permit it to make the same amount of gain as the experimental group. This control group was, on the average, brighter, over a year more mature mentally, and one grade farther advanced in knowledge than the experimental group. The gain of the experimental group did not take place at the expense of standardized progress. The photoplays were most effective in teaching about the interaction of

forces and events, historical personages and geography. They interfered somewhat with the learning of time relationships.

Three methods of presentation have been employed by Russell (148) with groups equated in intelligence. By one method the material was read twice to the class; by a second the students were to read the material at their normal rates over a period equal to that consumed by the teacher's reading to the first group; and by a third the pupils were to read the material twice at a normal rate, regardless of absolute time. Learning was tested by essay and true-false questions. With Grade V the teacher's reading was best and the pupils' reading with time constant was worst. In Grade VII the methods were roughly equal; and in Grade IX the teacher's reading was worst and the pupils' reading with frequency constant was best. The differences in the two higher grades are very small, however.

The lecture (auditory) and reading (visual) methods of presentation are equally effective in the acquisition of the content of psychology (Greene, 62). The fact that the high quartile in both intelligence and grades did slightly better with the reading method and the low quartile with the lecture method has already been noted. In learning Latin (Fenton and Hill, 45) the reinforcement of the book's presentation by pictures to illustrate the words proves beneficial; and in learning meaningful materials (Ewald, 43) the subjects who employed visual grouping did better than those who used an auditory grouping. Gudeff (65) discusses briefly certain physiological and anatomical aspects of the visual and auditory modalities and their relations to memory.

In interpreting such experiments one must remember that the influence of sensory modality becomes, under most experimental conditions, a problem in the method of presentation of the material, since presentation to a given receptor by no means insures that learning will go on primarily in terms of that modality. The subject may translate the material into any terms he is able to use.

The Grouping of the Material. The work presented under this topic is correlative with that discussed under the foregoing heading. The experiments there discussed were concerned with the objective presentation of the material to the subject by the experimenter; those here reviewed have to do with the units in which the subject handles the presented materials. Ewald (43) has attacked at some length the problem of the size of the groups or 'complexes' with which subjects can deal effectively in learning both nonsense syllables and meaningful material. Wide individual differences appear. Some

subjects, for example, can manage only 4 syllables as a unit; others, the best, can manage as many as 8. The best learners of both materials are those who apply grouping methods, who are 'complex-strong'; the poor learners are weak in ability to group. The size of the units which can be handled varies also with the nature of the material. Vogelsang's (173) better subjects were, likewise, able to deal with larger groups than were the poorer ones. The introspective data of an experiment by Martin and Fernberger (114) on improvement in memory span show that the improvement occurs at the level of organization into groups. The subjects experienced difficulty in handling groups containing more than five items.

Mnemonic Devices. Intimately related to the last section is the matter of mnemonic techniques. Belanner (7) publishes a graphic representation of his own visual number-form with cases illustrating its use in recalling numbers and dates. Spielrein (159) was able to duplicate, by organization into a pattern, the feat of a 'memory-artist' who could repeat a long list of words heard but once. He describes the difficulties and characteristics of such organization. Hegge (72) has called in question Spielrein's interpretation of certain of his illustrations, but agrees upon the importance of meaningful connection. Hosiasson (80) calls attention to the mnemonic value of classifying material to be learned and of associating it with one's already acquired experience.

Phenomenal Memorizing. The period's crop of phenomenal memorizers is hardly as luxuriant as was the one harvested in 1928, but they are still with us. Jaensch and Mehmel (86) describe an imbecile boy of eighteen who by use of eidetic imagery could give, with a mean reaction time of 7.8 sec., the day of the week of any date mentioned between 1920 and 1927. He can do the same for other years if the questioner will tell him the day of the week of any one date of the year in question. He gets the calendar for the entire year in eidetic imagery; no calculating is required. A case is reported by Phillips (134) of a low grade imbecile boy with specific memorial ability for words and tunes. Löser (108) records the case of a pupil with unusual visual memory for digits and words, an ability far in excess of the rating of his other associative processes. The man described by Friedländer (50) is possessed of a poorly integrated personality, but is able to repeat *in toto* the contents of the Sunday edition of a large newspaper which he has read but once. He is able to do likewise with speeches, repeating not only the words but the gestures and mannerisms of the speakers. The unusual

memory of the lightning calculator of whom Blachowski (10) writes is also in point. The use of essentially the same mnemonic devices as those described above appears in most of these cases.

The Influence of Attitude or Set. Jersild (89) raises the question of the influence of the mental activity aroused by a preëxamination upon subsequent learning. In five different experiments subjects were examined upon materials new to them and then reëxamined after a period of study. Equivalent controls were not preëxamined. The use of multiple-choice and essay questions for preëxamination resulted in consistently higher scores for the preëxamined group. True-false tests were either ineffective or detrimental. In so far as preëxamination arouses an activity attitude and stimulates industry it is beneficial. Analogous to these conclusions is that of Washburne (174) concerning the greater efficacy of questions placed at the beginning of a story than of those placed at the end. His results, also, are to be interpreted in terms of the set aroused by the questions.

Maso (115) compares the influence of active and passive attitudes upon learning. In one experiment, under the active attitude, the subjects were to sort pictures accompanied by their names into three designated classes; under the passive attitude they were given the pictures already sorted and were told to look at them attentively. In other experiments the memorial values of matching names and objects, attributes and objects, and looking up specified biographical information were compared with the corresponding values when the materials were presented already matched or gathered. The precise nature and extent of the active attitude and the amount of its differentiation from the so-called passive attitude are unknown, but the two attitudes yield somewhat different results. For a majority of the subjects the active attitude "is the better. The differences between the two are much larger on delayed than on immediate recall.

The problem studied by these three investigators is one which cuts across a large proportion of the other problems of memorizing and retaining, and overlaps, as do many such, the fields of thought and reasoning. The investigations of the effect of attitude have clearly indicated that learning alters with change of set. The quantification of set and the measurement of its influence along a qualitatively uniform continuum is a highly important problem of the future.

Memory Span Studies. Martin and Fernberger (114) report a rather marked improvement in auditory memory span following

continued practice. One subject improved 47 per cent, the other 36 per cent. The authors are careful to point out that interpretation of these results must be guarded. The span which is improved is that obtained by grouping and gains are based on the development of better methods. If, on the other hand, one means by memory span the range of items which can be reproduced after a single, ungrouped act of attention, the span is little, if at all, susceptible to improvement through practice. Martin and Fernberger gave their adult subjects two series daily for each number of digits from five until the span broke down. Young (181) has employed a somewhat different technique with subjects varying in age from 4-0 to 10-11. When the subject failed three forward auditory digit-spans at a given length, a series of that length was repeated to him until he had mastered it or had made ten failures. The subjects were taken as far as they could go before making the criterial number of failures. The average learning span was less than one digit beyond the immediate memory span. Thirty-eight per cent of the cases were incapable of learning beyond their digit-spans, 50 per cent attained a learning span of one digit greater, 11 per cent of two digits greater, and a few cases did still better. Revised norms for memory spans for digits, syllables and ideas have been published by Gillespie and Brotemarkle (56). Brooke (19) has compared the auditory and visual memory spans of clinical and institutional cases and has concluded that the latter are inferior.

Serial Position. Foucault (49) proposes to examine two different kinds of inhibition which appear within a memorized series: regressive internal inhibition, which is similar to retroactive inhibition save that the inhibiting factors arise within the series itself, and progressive internal inhibition, which refers to the detrimental influence of the earlier items of a series upon the later ones (proactive inhibition). He reasons that, as lists of words, once presented, are increased in length from 3 to 7, regressive inhibition will show itself in a lessened number of correct recitations of the initial item, then of the second item and so on, while progressive inhibition will have a similar effect on the last items. When adults are used as subjects, he obtains no very consistent results, but when 100 children are employed, clear serial position curves of the customary type, and of the kind required by Foucault's assumptions, appear. If the two kinds of inhibition work additively, the 4th word in a 7-word list should suffer the sum of the forgettings of the first and last words of the list, here 21. Actually its forgetting score is 81. Thus the

combined action of the two factors is greater than their sum. He explains this by assuming that succeeding words are weakened by progressive inhibition and are thus unable to resist regressive inhibition. In the middle of a list this condition obtains most strongly. Foucault's experiment is of the usual serial position kind, but his interpretations, which are the same as those previously made in outline by Woodworth and Poffenberger (1920), are important. They fit the obtained facts with some exactness and constitute an internally coherent explanation in terms of already familiar concepts. If we grant the validity of this hypothesis, the advantage of primacy is explained by freedom from progressive inhibition and of finality by freedom from regressive inhibition. In most of his series, as in the results of Robinson and Brown (1926), for example, primacy is superior to finality. It should be noted, however, that there are other factors as well as inhibition which may enter into the production of serial position values, such as positive association with position, a possible review, and a 'set' toward certain parts of the list, notably the first and last. In their study of associative blocking, Elkins and Krasnopolsky (42) find a progressively diminishing effect as the member common to two successive lists is displaced toward the middle and end of the series. They explain this in terms of the basically forward direction of associations.

The four factors, primacy, recency, frequency and vividness have been carefully examined by Jersild (88, 90). The material consisted of 70 narrative statements composing the biography of a fictitious person. Rotation of statements served to counterbalance differences in memory-value between the statements themselves, and each score for a statement under the influence of an experimental factor was calculated in terms of the scores for that statement when placed in the middle of the exercise and not subjected to any experimental device. Primacy (first position) has a high value, which is exceeded only by a frequency of 3, 4 and 5 repetitions and by one vividness factor. The second and third positions also have some advantage. Primacy is much superior to recency, regardless of what degree of each is compared. Willoughby's (179) data support the superiority of primacy and finality and confirm the current conclusions regarding serial position.

Frequency and Vividness. In Jersild's (88, 90) comparison of several factors of advantage, or modes of emphasis, in the learning of a biography spoken by experimenter to subjects, frequency and several devices to provide vividness were employed as well as the

conditions of primacy and finality. The nature and value of each may be shown most compactly by listing them in order of rank from best to worst in terms of percentage score: 5, 4 and 3 repetitions; "now get this"; primacy; 2 distributed repetitions, positions 11-60; second position; 2 distributed repetitions, positions 35-40; "Did you notice that?"; pause; 2 concentrated repetitions, positions 60-60; third position; recency, last position; loud; third from last statement; second from last statement; gesture; 2 concentrated repetitions, positions 10-10; bang; middle or normal; and slow. The range of percentage score is from 315 to 79. It will be noticed that the vividness devices tend to come toward the middle and lower end of the rankings, the frequency and primacy factors toward the upper end. In point is Willoughby's (179) low but significant correlation between recall and amount of practice upon the associations recalled.

The Results of Continued Memorizing; and Transfer. After preliminary practice, Heron (74) presented successively two separate lists of 6 paired syllables to large numbers of subjects under three conditions of relation between the members of the two lists: (A) all syllables different; (B) first and last pairs of the second list containing a syllable found also in the first list, and (C) the first members of the first, second, fifth and sixth pairs being syllables found in the first list. The conditions were presented in counterbalanced practice order. The second list was learned uniformly with fewer trials and fewer errors than the first list, and these differences were statistically reliable. This is viewed as a warming-up effect or a positive transfer lasting over only a short period. It had disappeared after an interval of one week. This disappearance, and the fact that practice effects and differences in the difficulty of the lists are cancelled out, are taken to validate the warming-up effect and to distinguish it from a transfer of specific habit-units. Elkin and Krasnopolsky (42) obtain small but consistent practice effects from the learning of four different and successive lists of 10 numbers each. When the first members of two lists are the same, difficulty of learning the second list is somewhat increased. Within limits an increase in the number of common members increases the degree of associative blocking. This blocking or negative transfer is less strong in 'mechanical' than in associative or logical memory. Brinkmann (18) attacks by new methods the old *Aussage*-problem of the trainability of observation and report. He uses the search method of Ach and concludes that such abilities can be improved by training. One good device is practice at observations which increase progressively in

difficulty. The most important condition for good performance is a suitable determination.

Orata (133) attempts a critical analysis of Thorndike's theory of identical elements. He first reviews numerous experiments on transfer of training and concludes that Thorndike's theory of identical elements is, for several reasons, invalid. (A) It is not supported by reliable experimental evidence; (B) logically, to say that transfer occurs in terms of identical elements is but to say that it does occur; (C) the theory rests upon a specialized, atomistic view of mind; and (D) it is incompatible with sound educational theory. Transfer takes place, his analysis of the literature is taken to reveal, "to the extent in which conditions favorable to transfer are present," which is scarcely enlightening. He proposes to reinterpret the problem in terms of a different theory of mind. Transfer is defined as "the extension and application of meanings to new problems or situations in such a way that we can deal with them effectively." Amount thereof "depends upon the extent to which meanings are identified and applied," and is "facilitated by concept formation." It is not possible to examine here Orata's interpretation of the experimental evidence. It is made, however, against the background of a theory of mind which is briefly presented, to defend which no systematic attempt is made, and in which appear many crucial items which are but categorically stated. The evidence against a narrowly specialized and atomistic view of mental functions may be convincing, but a more systematic work is needed to render acceptable the reinterpretation given, although it should be said, in fairness, that it contains many important comments upon the problem.

Correlations Between Different Materials. The important investigation by Garrett (53), whose conclusions regarding the relation between intelligence and memory have already been reviewed, involved computations of the intercorrelations between the memory and learning tests employed. The coefficients were for the most part low, around 0.21 as a central tendency, but when the proper statistical technique was employed, it was found that there was a common memory factor present which "is represented by an average intercorrelation which is almost certainly as large as .11 and may be as large as .17." Fischler and Ullert (46) have intercorrelated the performance of a large group of subjects on immediate memory for phrases, words, pictures, digits and geometric figures. The correlations, as were Garrett's, are low; and some are negative. Scrutiny of the tables of the two papers yields the tentative suggestion that the tests

which are most alike in form, in content, or in both, yield the highest intercorrelations.

In McFarland's (117) study of the relationship between speed and mental ability, the correlations between memory for serial impressions and other tests run in decreasing order from an $r.$ of 0.77 to one of 0.10, as follows: insight, maze running, form board, sentence completion, simple reaction, opposites, synonyms, discrimination and number sense. Every $r.$ save the last is 0.40 or better. In the complexity-of-task ranking, memory is well toward the top. Netschajeff (130) likewise reports high correlations between digit-span, sentence-span and six non-memory tests, as well as one of 0.66 between the two spans.

Low correlations have been found by Hartmann (71) to obtain between memory for words, reports on pictures and tests of perseveration. The learning of meaningful material (Ewald, 43) involves abilities different from those required in learning nonsense syllables. The subjects who did best with the latter did not excel with the former where, instead, the average learners of syllables did best. McGeoch (120) has intercorrelated the narrative and interrogatory report scores upon three different materials, the Binet object-card, a picture and an event. The average correlations between narrative and interrogatory score varies between 0.230 and 0.428 for correct items and between 0.095 and 0.203 for wrong items. When the same methods of reporting upon different materials are intercorrelated, the $r.$ s range from 0.076 to 0.431, the size of the $r.$ being a function of the materials, scores and methods of reporting. It is concluded that the ability to report is a function specific to the several factors involved.

The Reliability of Experimental Materials. No paper devoted specifically to a determination of the reliability of the materials used in work on memory has appeared, but reliability coefficients have been published, somewhat incidentally, by Garrett (53) and by Hartmann (71). Garrett's coefficients are as follows: visual and auditory digit-spans, 0.68 and 0.80; visual and auditory paired-associates, 0.95 and 0.90; logical memory, 0.60. Hartmann's are: recognition memory for words, 0.58, and report on pictures, 0.22. Garrett has also correlated the sum of eight memory-learning tests with the sum of eight identical tests, obtaining a result which is approximately 0.93. In general it seems that the reliability of some at least of the materials for experimentation on memory is higher than is that of the stylus maze, for example, in the field of skill.

Methods, Apparatus and Materials. Robinson (145) has published a classification and evaluation of the methods of practice equilibration. The method of completed practice is seldom used because, *e.g.*, the limit of practice is so often beyond experimental reach. There are three methods of uncompleted practice: (A) the method of predicted learning, which is usable when the experimental condition is known to work against the practice effect; (B) the method of the control group, which requires that *N* be large; and (C) the methods of counterbalanced order, which assume relative linearity and that the groups will be equally influenced by a given amount of practice, and which is trustworthy if several cycles yield consistent results.

Chou (27) contributes a useful bibliography and classification of exposure apparatus. He suggests the term *bradyscope*, first used by Esper, for all exposure devices used in work on memory, in contrast to the term *tachistoscope*, which is reserved for apparatus with very rapid exposure rates. He describes a new quadrant tachistoscope which can also be used as a bradyscope. An account of an automatic card feeder and catcher mechanism to go with this has recently appeared (Chou, 28). The use of a moving picture projector as a demonstration apparatus is recommended by Pillsbury (136). Its cost of operation is negligible and it can handle a wide range of materials.

Glaze (57) has made a valuable experimental classification of 2,019 nonsense syllables in terms of their associative values. Each syllable was exposed to each of 15 subjects for 3 sec. in a tachistoscope, the subjects having been instructed to indicate what meaning, if any, the syllable had for them. If all subjects gave a response word to a syllable, it was placed in the 100 per cent division with regard to associative value; if no subject had an association with a given syllable, it was given a rating of 00 per cent; and so on for each proportion of the 15 subjects for whom a syllable had associative value. The classified syllables are published in full. Glaze does not claim that all of the syllables have the exact associative value assigned, but only that those in the lower ranges are actually low and are numerous enough for most experimental purposes. The value assigned to a particular syllable is doubtless a function of the experimental conditions employed by Glaze, but his work is an important step in the calibration of experimental materials.

Boring (14) reports the character of, and some typical results from, certain demonstrational experiments in memory which he has

used for a number of years and which yield results illustrative of the facts about length of list, distribution, and meaningful vs. nonsense material, together with qualitative observations. Awaji (2) describes an immediate memory test consisting of pictures. Learning is tested by a modified form of recognition.

Applications of Data on Memory. For an experimental group, teaching situations which applied some of the principles of the psychology of learning were chosen by Laton (104), and this group was compared with a control in the teaching of which no conscious applications of psychology were made. The experimental group acquired more knowledge than the control by several methods of testing. Sell (151) includes extensive tests of memory and of observation in his inventory of the personality traits of a normal twelve-year-old child; and Netschajeff (129) has used memory performance as one factor in a coefficient to determine the extent of the harmonious relation between sensory and motor functions.

Miscellaneous. Under this head will be reviewed topics upon which only a single paper has been published, or upon which very briefly statable results have been obtained. (A) *The Rôle of Articulation.* Three hundred and twenty-seven subjects, grade and college students, learned 20-word lists under a verbalization condition, in which they were instructed to repeat the words by use of tongue and lips, and under a restricted articulation condition, in which they were instructed not to say the words. They were assisted in the restriction by a pencil held crosswise between the teeth. The averages consistently favor the learning under unrestricted articulation. Barlow (3) interprets his results in terms of a dynamic influence of articulation upon learning mechanisms.

(B) *The Memory Value of Absolute Size.* Newhall and Heim (131) report that the immediate recall value of advertisements is independent of their absolute magnitude. Three absolute sizes, 50, 100 and 150 square inches, were employed with large groups of subjects and adequate experimental conditions. (C) *The Influence of Exposure Time.* Newhall and Heim, in the above experiment, used exposure times of 2, 5 and 15 seconds, with the result that immediate recall increases considerably with each increase in exposure time. Elkin's (41) conclusion that number of presentations required varies inversely as the speed of presentation is concordant. (D) *Rhythm.* Elkin finds, also, that the more the rhythm of the presentation of lists of numbers and of nonsense syllables is broken,

the larger is the number of repetitions required for learning. (E) *Independent Learning*. Fourteen-year-old boys are able independently to comprehend and assimilate ordinary school material for themselves. They have, as well, good insight into their own learning methods (Herrmann, 76).

(F) *The Mnemonic Independence of Form and Color*. Pialat (135) has raised this question and has investigated it by means of nonsense words paired with variously colored forms. The relative importance of the two factors is a function of the experimental conditions, but under the normal conditions employed color plays a rôle secondary to that of form. (G) *Memory for Onomatopes*. With pupils of the fifth and sixth grades and with college students, onomatopes have less advantage in immediate memory than concrete words and more than abstract words, when each is read in lists of discrete words to subjects covering a wide range of intellectual ability (Stoke, 164). (H) *The Direction of Association*. Stoddard (163) had 328 subjects learn and recall immediately lists of 50 paired French and English words under various conditions of direction of association. With learning order disregarded, the ratio of equivalents for French words recalled to equivalents for English words recalled is two to one; regardless of the order of learning, the unfamiliar French calls up more correct associations than the familiar English.

(I) *The Influence of Alcohol*. According to Kurka (102), alcohol in varying amounts increases the number of errors and the variability of records in the learning of nonsense syllables. (J) *Neural Conditions of Acquisition*. De Mortier (127) discusses the neurological conditions of amnesia of fixation and concludes that bilateral destruction of the prefrontal areas in man produces such amnesia. For mnemonic acquisition, also, the paths from the prefrontal to the posterior cortical areas must be intact. (K) *Memory of Pronunciation in Aphasia*. Noica (132) states that motor aphasia is especially characterized by a loss of memory for pronunciation of words. Loss of power of evocation also occurs. (L) *Imagery in Memorizing*. Garfunkel (52) has examined the relation between eidetic imagery and immediate memory for sentences auditorily presented, and finds no uniform relation. The memory performance of a feeble-minded boy with eidetic memory has already been mentioned (Jaensch and Mehmel, 86). Forsberg (47) stresses the importance of the relation between image-type and the material to be learned. (M) *Motivation to Study*. One of the major conclusions of Craw-

ford's (32) excellent survey of student opinion and activity is that motivation, appreciable purpose, is one of the major factors in the academic learning of students. A discussion of motivating factors is given.

(N) *The Influence of Information on Memory for Pictures.* Droba (40) has made an interesting experimental analysis of the effect of varying amounts of information about pictures of paintings in the Art Institute upon their recall. Some information is better than none, and the efficacy thereof varies with the amount and the exposure time. In general the information most favorable to recall is the name of the picture, the last name of the painter, and a short sentence about him. (O) *Attention and Memorizing.* Schorn (149) finds that children report much less well when the material was received simultaneously with the occurrence of another activity than when it is presented alone. Divided attention, presumably, is deleterious. With this the work of Sterzinger (162) agrees. Meyer (126) has shown that total attention is superior for formal and quantitative relations, while discrete attention is better on the qualitative side. Sengupta and Bose (154) emphasize the importance of definitely directed, as opposed to indefinitely directed, attention. Wide individual differences appear in the influence of distraction upon the learning of nonsense syllables (Weber, 175). (P) *Racial Differences.* According to Graham (60) whites are superior to negroes in rational learning. Gray and Bingham (61) conclude that white children are superior to colored in musical memory by the Seashore Test. There is, however, likelihood that the superior intelligence of the whites is a major factor in both of these cases, as in Lightfoot's (106) finding that colored children have short memory spans. Netschajeff (130) finds small memorial differences between children of five different nationalities.

II. RETENTION

It is interesting, though scarcely gratifying, that there has been for years a clean-cut selection in research upon retention in favor of memory problems. Within the general field of memory, retention has been studied almost, if not quite, as systematically as has memorization; while in the field of skill, on the other hand, very little systematic work has been done upon retention. The reasons lie embedded in the history of research upon learning. They can hardly be examined here, but the facts are deserving of comment.

The Curve of Retention. Bassett (4, 6) has made an extensive

study of the retention of history in the 6th, 7th, and 8th grades. Carefully constructed tests were given at 4-month intervals after the completion of a given course. The retention curves, which run from 0 to 16 months, show the usual negatively accelerated form, but the absolute amounts of forgetting are much less than those of the classical studies of the foregetting of nonsense syllables and other disconnected material. After 16 months an average of only 28 per cent of the material had been lost. Interpretation of these results must take into consideration the fact that the tests were given to the same subjects after each interval and that during the meantime these subjects had been studying other courses in history with the resultant inhibition or facilitation which may have followed such study. Bassett's data, however, represent well the facts for actual school conditions. Great care has been taken to render the material representative and trustworthy for these conditions. Another and similar investigation by Brooks and Bassett (20) with junior high school pupils yielded the same negatively accelerated retention curve and the same small absolute amounts of forgetting. In a year's time, *e.g.*, 23 per cent of the American history known at the close of the semester during which it was studied had been forgotten. Johnson (93) has computed the percentages of retention of elementary botany over periods of 3, 6 and 9 months and, by a less accurate method, after 15 and 27 months. The negatively accelerated curve form appears, but the absolute amounts of forgetting are much larger than those obtained by Bassett and by Brooks and Bassett. The difference, over and above that intrinsic to the difference between the materials, may be explicable in terms of the fewer subjects and the less exactly comparable and exhaustive retests available to Johnson. In a preliminary report, Worcester (180) describes an experiment designed to measure the retention of high school algebra. The initial results indicate high retention.

Whitely and McGeoch (177) obtain a curve of retention for poetry which falls abruptly from immediate recall to recall after 30 days and very gradually from that point to 120 days. The drop from 30 to 90 days is insignificant. After 120 days 28 per cent of the amount originally recalled is still retained. Anderson and Jordan (1) have plotted retention curves for Latin words (paired-associates) and phrases. Retests were made after one day, one, three and eight weeks. After one day the Latin stimulus words were given in the order in which learned; on later recalls they were presented in a changed order. Retention of words falls off irregularly from an im-

mediate recall of 74 per cent to one of 54 per cent after 8 weeks. Phrases fall correspondingly from 60 to 50 per cent. The negatively accelerated curve form holds throughout. Krueger's (101) recall and saving records for the retention of lists of 12 monosyllabic nouns after 1, 2, 4, 7, 14 and 28 days present the customary negative acceleration, but the amount of forgetting and the sharpness of the deceleration vary inversely as the degree of learning. Griolet (63, 64) attempts to show that forgetting is proportional to a root of the elapsed time. The particular index is dictated by the experimental conditions.

There have been few reported cases of reminiscence, the striking inversion of the usual retention curves which Ballard made famous. Some evidence of the phenomenon has been obtained by Foster (48) who counted the number of words given on the first relearning repetition of stories but never given on any learning repetition. Individual differences appeared: a few subjects gave no new words, others gave many, one averaging 20.8. Bloor (12) is inclined to ascribe the reminiscence effect to subconscious activity, an explanation which must needs be given further systematic elaboration to be significant.

The Influence of Interpolated Activity. The blocking of measurable retention by interpolated activities has far-reaching implications for the understanding of forgetting, and one of the most urgent problems of this blocking effect is that of the influence of the similarity obtaining between the original and the interpolated activities upon the amount of the inhibition. This has been studied by Harden (70) and by Cheng (26). The former takes her problem from the failure of Robinson's (1927) results to conform to the hypothetical Skaggs-Robinson relation between similarity and degree of retroactive inhibition. This failure may have been due to the fact that Robinson employed consonants (similar materials) in both halves of his 8-member memory spans, at the same time varying degree of identity. While this method carries degree of similarity from identity downwards, it does not reach farther than something like a median degree, even with no consonant in common, because of the identity of the type of the material. Harden proposes to use consonants in the first half of the span and in the second a number of digits corresponding inversely to the degree of similarity desired, thus introducing non-identity of items and dissimilarity of material into the second half of the span. Thus in her experiment the greatest similarity between the two halves is attained by using dif-

ferent consonants in both, and the least similarity by using four digits in the second half. In the three intermediate degrees one, two and three digits are used. The factors of order, position and practice are controlled. The conditions of presentation are different from those of Robinson, but the implications of the results are perhaps little affected by this difference. The percentage of correct recall declines from four items in common to one item in common and mounts sharply when there is no common item (assigned zero similarity). When the Robinson-Harden data are combined, a curve resembling in form the hypothetical relation appears save for the difference that the right section of the curve mounts sharply instead of flattening out.

Cheng has approached in two experiments the problem of the influence of similarity, using nonsense syllables as both original and interpolated material and achieving three degrees of similarity by having the interpolated syllables related to the original syllables of corresponding position to the extent of no common letters and one and two letters in common. In the first experiment the intra-syllable position of the common letters was varied to cover all positions. In the second experiment only certain intra-syllable positions were used. All interpolated conditions produced some retroactive inhibition, the degree of which varied with the experiment and with the method of measuring retention. Measured in terms of recall, retroaction at first increases and then decreases as degree of similarity increases from zero to two identical letters, while in terms of saving retroaction exhibits only a very slight tendency to decrease with increase in similarity. In the first experiment identity of the vowel had less retroactive effect than did identity of either consonant, and identity of the first and last letters had less effect than did identity of the two first or the two last, which are about equal in effect.

The evidence in favor of similarity as one of the conditions of retroaction is accumulating, as is support for the general correctness of the Skaggs-Robinson hypothetical curve under certain specified conditions. The experimental work of the last few years has carried the problem well beyond the speculations of the early investigations, but we are still far from the point at which a satisfactory generalization can be made. Experimenters have worked, perforce, with narrowly specified kinds of similarity in arbitrarily defined degrees. In the work reviewed, for example, Cheng has kept within the circle of identical letters in 3-letter syllables, that is, within a narrow range of formal identity. Harden, extending Robinson's technique,

has retained a certain kind of formal identity but has moved beyond a single continuum of material, and has thereby raised puzzling theoretical questions. She would designate the series BLFP7469, *e.g.*, as one in which the similarity between the two halves of the span is zero, and the series BLFPXCJD as one of maximum similarity because of the continuity of the type of material. Actually, of course, the quantitative relation between the two degrees of similarity is unknown, and to plot similarity thus defined in the same continuum of material, though really passing in the middle from one continuum to another, is an arbitrary procedure. Harden is, of course, aware of this fact and can appeal to the rational extension of the Robinson curve which her results provide. It is both remarkable and important that so clear a continuity of results emerges from so arbitrary a definition of similarity, and further explorations of other kinds and degrees of similarity may permit eventually a generalization.

Jenkins and Dallenbach published in 1924 the results of an important inquiry into the relative amounts of forgetting after sleep and after waking. Forgetting after sleep was much less with all intervals and decreased as the intervals lengthened from 1 to 8 hours. This fact points to the interpolated activity of the waking hours as a blocking factor. Dahl (37) has repeated the Jenkins and Dallenbach experiment with 14 subjects, using 12-item lists of nonsense syllables and of figures and testing retention by the recognition method. The sleep condition is consistently better when only errors in the form of failures to recognize are considered; it is superior only after 4 and 8 hours (not after 1 and 2) when errors in the form of recognizing new material falsely, and total errors, are counted. The facts corroborate, in the main, the results of Jenkins and Dallenbach, but Dahl refuses to interpret poorer recall after waking as a case of retroactive inhibition. He thinks that the results say nothing about what happened to the material *during* sleep or *during* waking, but are rather a function of the subject's condition at the time of recall. After one and two hours of sleep the subjects were very sleepy and in no condition to recognize clearly, hence they did somewhat worse (in that they wrongly recognized confusion materials) than after waking. After longer periods of sleep they were in better condition and did better than after corresponding periods of waking. That there was some difference in the subject's condition after the different intervals is taken to be indicated by the fact that the tendency to give more 'yes' answers after sleep than after waking was much greater after 1 and 2 than after 4 and 8 hours.

If we consider only Dahl's results, there is no way of adjudicating the retroaction vs. condition-at-the-time-of-measurement issue. When, however, it is remembered that the Jenkins and Dallenbach subjects would, when awakened by the experimenter, go to the next room, recall in a very sleepy condition, return to bed, and be unable to remember anything of the experience the next morning, it seems hardly likely that there was a greater 'fitness' for recall after the sleep intervals. These subjects recalled, also, under a more exacting method than did Dahl's. Spight's (160) evidence for more effective completion of learning after a period, the major part of which was spent in sleep, than after an equal period of waking, agrees factually with the findings both of Dahl and Jenkins and of Dallenbach. Since her subjects did not resume the learning until some time after waking, the results can not be interpreted as the outcome of any immediate condition attendant upon being awakened, although the general fitness may have been greater. The considerations mentioned above, together with the coherence of these facts with those concerning inhibition, create a presupposition in favor of the interpretation of the better retention after sleep as an outcome of decreased blocking by interpolated activity.

McGeoch (122) has studied the extent to which retroactive inhibition is a function of the degree of learning of the interpolated material. Nine-syllable lists were learned by the anticipation method to 6, 11, 16, 21 and 26 repetitions, and a constant interpolation of 11 repetitions of a list of equal length was used. In terms of recall, the relative amount of retroaction varies inversely as the number of presentations given to the original material. With as many as 26 presentations, and the resulting high degree of overlearning, there is an inhibition of 46.2 per cent. When retention is measured by the saving method, relative retroaction again varies inversely as the number of repetitions, but the curve of inhibition plotted against repetitions shows a marked negative acceleration, falling from 108.5 per cent at 6 presentations to 5.3 per cent at 26 presentations. Thus by both methods of measurement retroaction decreases as learning increases, but the amount of the decrease is a function of the method, recall being much the more susceptible to inhibition.

Foucalt's (49) interpretation of certain aspects of the serial position curve in terms of retroactive inhibition has already been discussed. Gibson (55) makes incidental mention of the same phenomenon. His subjects complained that as each succeeding figure

of a list appeared it 'blotted out' the preceding figure, until at the end only the last one or two could be remembered.

The Influence of Degree of Learning. This problem has taken on renewed importance from the recent discussions of the general importance of frequency for both learning and retention. Krueger (101) has made an important study of the influence upon retention after 1, 2, 4, 7, 14 and 28 days of degrees of learning represented by the percentages 100, 150 and 200. Lists of 12 monosyllabic nouns were learned and relearned by a form of the anticipation method. As degree of learning increases from 100 to 150 per cent, there is nearly a 50 per cent decrease in retention after an interval of one day, and as the interval between learning and relearning increases the relative advantage of the greater degree of learning increases. As degree of learning increases from 150 to 200 per cent, the corresponding increment in retention is usually less than one-third, and the proportion bears no consistent relation to length of interval. In general his work shows that overlearning of at least 50 per cent is highly economical for retention over intervals up to 28 days, and that the larger the interval the greater is the economy. Further degrees of overlearning are uneconomical for most intervals. It should be noted that Krueger's results contradict those of Luh (1922), a fact which may be due to differences in experimental conditions.

Ebbinghaus concluded that each repetition after learning to the threshold has been accomplished has approximately an equal effect upon retention, in that each addition of three learning repetitions means one repetition saved in relearning. Cuff (34, 35) has called this conclusion in question. He had groups of college students learn 12-item lists of consonants, digits and syllables, then take either 4, 16, or 28 additional repetitions, and relearn after 24 hours. The results exhibit no definite relation between additional repetitions and saving. Overlearning may aid, hinder or have no effect upon retention. It cannot, however, be concluded that the traditionally quoted conclusion of Ebbinghaus' is completely overthrown, since the conditions of the two experiments are in several ways widely divergent.

The Nature of the Material. It is an often-repeated generalization in discussions of retention that acts of skill are much better retained than are memorial materials. McGeoch and Melton (124) have examined the literature upon which this generalization is, presumably, based and have found numerous exceptions. They have then compared the retention values, after a seven-day interval, of three mazes of varying degrees of difficulty and nonsense syllables in lists

of 8, 12 and 16, when each maze and each list is learned to a criterion of one perfect repetition and relearned to a criterion of three successive perfect trials. The relative retention values of the two materials are functions of the particular maze and syllable list compared, of the method by which retention is measured, and of the relearning criterion employed. In terms of trials the syllable lists are unequivocally superior. In terms of time and errors the mazes excel in a majority of the comparisons, although in terms of both scores at least one syllable list is equal to or better than at least one maze. Further, when the mazes are superior, the amount of their superiority is by no means as great as that usually assigned to motor skills. The only very large difference in retention values occurs with the criterion of trials, where the syllables greatly excel the mazes. The difference between these results and those showing relatively higher retention for acts of skill is interpreted to lie in the factor of degree of learning.

Different kinds of historical knowledge have different rates of forgetting (Bassett, 4). Knowledge which is concrete and personal tends to be well remembered. Names and geographical locations are forgotten rapidly. Anderson and Jordan (1) find that the closer the formal relations between Latin words and their English equivalents the better the retention. In point also are the experiments reviewed under the curve of retention. Although comparisons of the retention values of different materials, when such values are derived under varying conditions, can have suggestive importance only, it seems that logically connected subject matter, such as history, botany and poetry, is better retained than memorial materials have often been thought to be.

Affective Tone and Retention. Meltzer (125) has summarized in tabular form 26 experimental papers on this problem and has criticized them briefly. The major shortcomings are: (A) too few subjects; (B) unreliable method-questionnaire; (C) conditions too narrowly restricted and insufficiently life-like; (D) unwarranted assumptions tacitly made, such as the assumption that the subjects have experienced equal numbers of pleasant and unpleasant activities; and (E) forced or unwarranted interpretations of results. He suggests that the problem should not be stated in terms of the influence of feeling upon memory, but rather in terms of correlation.

Data regarding the earliest childhood memories of both adults and children have been collected by Blonsky (11) and regarding those of adults by Gordon (58). The content of the memories of Blonsky's subjects was preponderantly unpleasant, and the older the

memories the greater the tendency for them to be unpleasant. Misfortune turns out to be the weightiest mnemonic factor. Upon reading Blonsky's paper one wonders if the content of the memories of persons in other parts of the world would agree with those of the inhabitants of Moscow; the factor of the experiential ratio of pleasant to unpleasant experiences must always be considered. Blonsky's conclusions find, however, emphatic support in Gordon's canvass of 750 students, which revealed that unpleasant memories bulk largest for both men and women. Neither study lends support to the Freudian doctrine of the repression of the unpleasant, although neither, as it stands, can be taken as a denial thereof.

Jones (94) offers a criticism of Smith's (*The Measurement of Emotion*) work, and repeats it with certain words and extends it. The mean galvanometric deflections obtained by Smith and by Jones correlate highly and agree in the placement of the highest and lowest words. The memory values for critical and non-critical words lead Jones to suggest a theory of the bidirectional influence of emotional tone upon memory. Increased emotional intensity may affect learning either favorably or adversely, depending upon whether the emotion is 'positive' or 'negative.' Words which had been evaluated for affective tone on a 5-point scale by 150 students were learned by 20 girls for Chaney and Lauer (25). Pleasant, unpleasant and neutral words were recalled about equally, and all were slightly inferior to the mixed lists. Selz (152) believes that there is a positive relation between pupils' liking for a subject and their recall thereof, but the experimental conditions are not clean-cut. Cuff (35) reports a slight tendency for subjects with the lowest affectivity and idiosyncrasy scores on the Pressey X-O Tests to have the largest percentages of saving. Guiraud (66) speaks of the induction, in abnormal states, of series of recollections under the influence of an affective tendency.

Repeated Reproduction. Departing from a critical examination of Wulf's (1922) study of ideational change, Gibson (55) analyzes reproduction of geometrical forms at successive sittings and after periods of five weeks and a year. The reproduction of a visually perceived form is frequently changed in the direction of a familiar associated object, and the reproduction of one figure may be changed in the direction of another figure if the two have been associated in consciousness. Gaps or breaks in the contour are partially or wholly closed up in reproduction, or else the figure 'falls apart' into separate units. These facts are interpreted as evidence for the existence of perceptual habits. Reproductive change depends upon the manner

of apprehension. Zillig (182) asked from her subjects written reports on an event immediately after its occurrence, on the next day, and after a week. She divides the subjects into three types with respect to their behavior on successive reproductions: (A) the constant type which reports repeatedly in the same manner; (B) the variable type which alters its reports on successive occasions; and (C) the intermittent type which varies its report from correctness to incorrectness.

Studies of Post-hypnotic Function. Strickler (165) has experimented systematically upon the forgetting of geometrical character-nonsense syllable paired-associates learned in the hypnotic trance and in the normal waking condition. Four subjects who showed complete waking amnesia for trance events were used. The net recall amnesia for the associates is about 80 per cent complete, while the post-hypnotic relearning amnesia is only about 50 per cent complete. In spite of excess total learning in the trance series, material thus learned suffers nearly twice as great a deterioration as does normally learned material, after a lapse of twenty-four hours. Kellogg (97) has measured the duration of the post-hypnotic suggestion that the respiration rate be varied in a certain way. The same suggestions are given to a normal group and are not significantly forgotten by them, while in the case of the hypnotized subjects the influence of the suggestion decreases during the first three weeks.

Testimony. There have been several papers and one book devoted to the general problem of testimony. Tesoro (166) gives a somewhat popular description of the factors of importance in evaluating testimony, and Claparède (29) discusses unreliable testimony. Gorphe (59) analyzes the conditions which must be taken into account in the education of ability to report. Révész (142) and Busemann (22) report cases illustrative of the unreliability of children's testimony upon sexual matters, and Plaut (139) examines in some detail what can be expected from the testimony of young psychopaths. The same writer (138) emphasizes the need for a study of the psychological conditions of the testimony of adults. Selz (153) and Kafka (96) report cases. Other studies of the ability to report have been reviewed in connection with specific problems.

Miscellaneous. Under this heading will be reviewed topics upon which but a single paper has appeared or upon which the results permit brief statement. (A) *Correlations Between Learning and Retention.* The correlations between initial history tests and tests after 4, 8, 12 and 16 months are high, 0.797 and 0.840 (Bassett, 4).

This is corroborated by Brooks and Bassett (20). Anderson and Jordan (1) obtain coefficients of 0.75 and 0.79 between immediate and delayed recalls of paired-associates after 3 and 8 weeks, respectively, and Johnson (93) reports *r.s* of 0.52 and 0.67 between the amount of botany known at the completion of the course and relative retention. (B) *Correlations Between Retention and Other Factors*. Bassett (4, 6) gives extensive tables of coefficients of correlation between the retention of history and other factors. Their nature can only be indicated here. Subject preference, interest and effort, and reading comprehension, *e.g.*, correlate positively with retention. Anderson and Jordan (1) report that immediate recall and recall after eight weeks correlate 0.46 with silent reading and 0.20 with word knowledge, and Bassett (4) finds MA and retention positively related.

(C) *Distribution and Retention*. Clark (31) has demonstrated that, for the retention of sonnets, duration of learning periods has more influence than duration of rest intervals, and that study periods decreasing in geometric progression, when compared with intervals of equal length, will reduce considerably the rate of forgetting. (D) *Sensory Modality and Method of Presentation*. Vértés (172) compared the memory of deaf-mutes and normal children, finding the deaf-mutes superior with words designated by touch images and inferior in words depending upon audition. There is no great difference in memory for things. In the Tilton and Knowlton (169) study of the effect of photoplays, the experimental group retained more of relation, person, and place knowledge, and less of time knowledge, than the control. In Greene's (62) comparison, lectures resulted in better retention than did reading; while the three methods of presentation compared by Johnson (92) are roughly equal.

(E) *The Influence of Age*. The data published by Thorndike and his associates (167) make it doubtful if forgetting is greater in the later than in the earlier adult years. Bassett (4) records a negative correlation between CA and retention. (F) *Infantile Memories*. Hadfield (67, 68) contends that memories of infantile experiences can be accurately revived and discusses the reasons for this contention and some of the errors likely to arise in the interpretation of such memories. The average ages at which the earliest memories of Gordon's (58) subjects are dated vary from 3.5 to 3.79, and Blonsky (11) gives 4.2 as the corresponding figure for his adults. The mean age of earliest memory for his subjects aged eleven to twelve is, however, 2.5. Some members of each group have memories which are referred to the first year of life. (G) *The*

Influence of Unrecalled Material. Campos and Radecki (23) and Radecka and Radecki (140) present evidence which they interpret to mean that 'forgotten' words may, nevertheless, influence voluntary associations, while having little influence upon free associations. (H) *Sex Differences.* After all intervals up to sixteen months, boys are slightly superior to girls in history retention (Bassett, 5). This superiority seems, however, to be a function of the material. The boys excel in knowledge about war and fighting and geographical places; the girls are superior in history treating of domestic relations and home life.

(I) *Rhythm.* Rhythmically learned material is retained considerably better than material of which the rhythm had been broken during learning (Elkin, 41). (J) *Recognition and Confidence.* Seward (155) sought to determine the extent to which speed of redintegrative response serves as an index of confidence. Whether positive or negative, the more accurate response is quicker and more confident than the inaccurate. When two responses are equally accurate, the negative is the faster and the more confident. Recognition time and confidence are not, however, sufficiently correlated to permit the use of the former as a measure of the latter. Jersild's (91) data indicate that confidence regarding true-false answers is a personal factor which is relatively constant, but which is no guarantee of the accuracy of the performance. (K) *The Influence of Attention.* With the transition from immediate to delayed measures of retention, the differences between discrete and total attention become neutralized (Meyer, 126). (L) *Tonal Memory.* König (99) reports that wide differences exist in tonal memory functions corresponding to differences in musical training. The trained may excel by large amounts. (M) *Errors in Reproduction.* Maity (112) puts forward the thesis that errors in reproduction are important for the study of the mental characteristics of learners, and makes an analysis of such errors. The two major types are lapses and perseverations of wrong items, both of which are made by poor learners much oftener than by those who are good learners. (N) *Cure of an Amnesic Case.* Tucker and Shield (170) describe the case of a man who had had several amnesic periods with a complete change of personality. An operative cerebral decompression was followed by a long period of normality.

III. GENERAL

Historical. The three histories of psychology published during the last two years contain valuable discussions of early work on

memory. Boring's (15) masterly volume, the quality of which any science could be proud to have to its credit, has a wealth of material for him who would understand the historical development of experimental research upon memory. Specifically in point are his sections on Ebbinghaus and on G. E. Müller; but no specific enumeration can do justice to the magnificent perspective in which Boring has marshaled the problems of experimental psychology, memory among them. Murphy (128) has a good chapter devoted to "Early Studies of Memory," in which Ebbinghaus receives the lion's share. That psychologist's subjection of learning and forgetting to quantitative treatment is evaluated as "probably the greatest triumph of original genius in experimental psychology since Weber," an interpretation which Langfeld (see 128) styles "a bit strong." References to work on memory occur frequently in Pillsbury's (137) *History*, Ebbinghaus and Müller again being given major treatment.

General Textbook Chapters. No attempt will be made to present under this caption a review of the chapters on memory in all of the textbooks published during the period of this review. Such an attempt would have to include texts in educational and applied, as well as in general, psychology and would run into more pages than should be given to brief accounts intended primarily for beginning students. A few books, however, present new points of view which demand special mention. Some of these will be treated also in the following section on contributions to theory. To the extent that the redintegrative paradigm is both a fact of nature and the framework (as well as most of the structure) of learning, Hollingworth's (77) whole book is based on and permeated with the principles of learning and memory, a mode of treatment which is one of the first overtly to recognize the systematic pervasiveness and generality of the concept of learning. His specific treatment of memory is stimulating and unique. Against the background of a configurational point of view, under the aegis of "least action," Wheeler (176) devotes three chapters to an account of learning in which there is much that is both new and important. Dashiell (38) gives less space to memory, but makes a thoroughgoing effort to give it objective status. Each of these three authors introduces new material, cites contemporary experiments, reinterprets old ones, and departs refreshingly from the hackneyed recitation of traditional facts into which textbook writers have sometimes fallen.

Books and Pamphlets. Bode's (13) *Conflicting Psychologies of Learning* particularly concerns itself with the implications for educa-

tion of various theories of mind. It has little or nothing to do with theories of learning in the technical sense, but deals rather with the wider context which determines the framework within which the specific theories of learning must be framed. The chapters on association and reproduction in Köhler's (98) *Gestalt Psychology* are critical analyses of prevailing viewpoints and constructive presentations of the *Gestalt* interpretations. The whole book provides the setting for a new theory of memory.

In a book which, though elementary, has the merit of both critical acquaintance with the historical and systematic relationships of memory and of fluid and engaging style, Janet (87) has set forth the relations between memory and time. The end of memory is to triumph over time and absence, which in themselves are prior to memory. Repeated use is made of illustrations and principles from the data of mental abnormality.

Burton (21) writes a general text for normal school and college students, and Wiley (178) lists in pamphlet form some 'practical' suggestions to students. Crawford's (32) survey of student opinion and of student activities has considerable practical significance. Markey (113) reviews work on the acquisition of symbols, which lies on the borderland between memory and skill. Forsberg (47) has surveyed some of the literature on memory and has presented experiments and conclusions of his own regarding the problems of understanding, learning and memory. Emphasis is placed on a study of 'natural' learning.

General Surveys of Learning and Retention. In Murchison's *The Foundations of Experimental Psychology* appear two chapters on learning which are of the first order of excellence. Lashley (103) surveys the nervous mechanisms involved in learning and treats of the phylogenetic development of learning capacity, the relation between learning and neural structure, the nature of central integration and the major "laws of learning." Hunter (82) writes on experimental studies of learning and summarizes the existent data upon the active problems of learning and retention. Both chapters include, also, work on the acquisition of skill and, to some extent, upon animals. In each the emphasis is on experimental fact only. Each author organizes and interprets critically and clearly. In Robinson and Robinson, *Readings in General Psychology*, McGeoch (123) has written a section on experimental studies of memory. He has also described (118) some of the major conditions of human forgetting.

Skaggs (157) offers a systematic classification of inhibition at the descriptive and psychological level. Voluntary inhibition may be of images or ideas, sense-presentations or of motor activity. Involuntary or passive inhibition has four varieties: (A) that operative at the time of learning, including associative, retroactive and affective inhibition; (B) that operative during recall, which may be cognitive or affective; (C) sensory inhibition, including attention-inhibition and reciprocal sensory inhibition; and (D) motor inhibition, as when a stimulus stops a movement. Retroactive inhibition and retrograde amnesia are held not to be the same.

Three papers on psychology and the law of evidence have been written by Hutchins and Slesinger (83, 84, 85) in order to make explicit the psychological assumptions of the law and to apply critically the data of modern psychology. In each paper certain assumptions and practices are evaluated in terms of the experimental facts upon memory, among others. The psychological sophistication exhibited in these papers marks a notable advance over the older slipshod acceptances and uninformed polemics which have so often attended efforts to apply the data of experimental work in psychology to an evaluation of testimony.

IV. THEORETICAL

The Nature of Learning. The theoretical treatments of this problem are, for the most part, sharply sundered by the insight vs. frequency issue in some of its forms. For Köhler (98) organization must be accepted as an original aspect of experience; association is but an after-effect of this organization. Contiguity is discarded on the ground that the law takes no account of the properties by virtue of which the contiguous events are associated. Such a statement has no counterpart in physics or chemistry. For him, ". . . neighborhood in space and time influences association only insofar as it determines organization," and the latter is either spontaneously present or may be created intentionally. A list of syllables, *e.g.*, is not learned because of the successive contiguity, but because the subject creates an organization intentionally. Such learning is unnatural, for in most of the acquisitions of everyday life organization is spontaneous. This view of learning is an antidote to the supposedly-current atomism, but for explanatory power it rests too heavily upon its postulates. In all cases where spontaneous organization occurs there is no need for learning, since it is already there as a given, and only retention remains a problem. Where organization must be inten-

tionally created learning enters, but here nothing is said regarding the properties of either situation or learner which render such creation possible. Surely one can demand as validly that these properties be specified as can Köhler that the law of contiguity define those of contiguously associated items A and B. Either type of organization, spontaneous or natural, must be accepted with natural piety wherever found, in which case the whole of memorizing either rests on a postulate or else the properties whereby in particular cases organization is possible must be specified. These criticisms do not, of course, apply on the level of the phenomenal description of mental life at any given moment, but only to the systematic interpretation of the flow of events.

A majority of Wheeler's (176) treatment applies more directly to the learning of motor acts than to the acquisition of memorial materials, although he clearly intends to apply the same principles to both. He regards learning as "behavior in terms of which the individual extends his insight into a given situation and increases the complexity of his actions with respect to a certain goal. When memorizing has occurred, one does not use fewer cues (*cf.* Hollingworth), instead he uses more, but they may have changed from the easily detected to the difficult to observe, and will have become better organized. Further, we do not learn by experience, unless we have "sufficient insight into the experience to profit by it, in which case it is the insight into the experience which measures progress, not the experience into which one has insight." Repetition is valuable only in so far as it induces maturation. Frequency "bears the same relation to maturation and learning that time bears to growth." Stimulus-pattern and maturation are the major factors in learning.

Much more would need to be said about this, as well as about the other points of view described in this section, to give a well-rounded picture; and far more would have to be said than can be written here if a fair attempt at evaluation were to be made. Briefly, however, Wheeler's insistence that insight is basic and that maturation, not frequency, is of importance, places the burden of learning outside of the range of antecedently operating principles and properties. It takes the developed insight as final, when insight is description after the fact; and it depends on maturation when such maturation is, in a majority of the experiments upon memory, hypothetical at best.

The learning of the relation of opposition by children aged 5.0 to 7.6 years has been studied by Kreezer and Dallenbach (100) and has yielded results which bear directly upon this problem. No subject

learned the relation gradually; it came suddenly with the acquisition of 'insight,' and once learned was not forgotten. The subjects learned the relation as such and not as a specific response. The existence of this type of learning is cited as evidence of the inadequacy of trial-and-error theories. The law of exercise, even though eked out by assimilation, is interpreted to be futile, as is recency, to explain the results. Frequency, recency and assimilation should have led to a continuation of the wrong response, and neither can have anything directly to do with the sudden appearance of the correct response. This experiment constitutes a striking example of sudden learning of the insightful variety, but the authors offer insight as a description, not as an explanation.

The experimental results of Kreezer and Dallenbach offer one reply to certain of Lund's (110) criticisms of the *Gestalt* insistence that learning proceeds, not by trial-and-error and the gradual selection of correct adjustments from random movements, but by sudden insight. Lund puts forward four criticisms: (A) it is a question whether sudden drops in the learning curve are typical; (B) even in Köhler's report of the learning of apes, some gradual perfecting appeared; the apes did not sit and deliberate until 'insight' came; (C) such insight as appears may be a case of transfer from previous acquisitions through trial-and-error; and (D) Köhler's preferred illustrations of sudden insight are cases in which the right act either does or does not occur. Without aiming at an evaluation of the two theoretical positions involved, it may be pointed out that the Kreezer and Dallenbach experiment presents one case in which there is no gradual acquisition, although in one form such would have been possible, and in which it is difficult to see how transfer of the kind described could have operated. Whether such learning is typical is another matter and one which research must decide.

Consonant with Lund's critique is a paper by Razran (141), who has examined nearly 200 original reports on the conditioned response and on negative adaptation and who presents them in a series of generalizations. He raises the question whether the more complex forms of learning proceed according to the known principles of conditioning. He thinks that they can be interpreted thus: poem-reciting is "adequately accounted for by proprioceptive and chained conditioning in which the response, or rather the proprioceptive part, of the first pattern becomes conditioned to the response of the second pattern, the response of the second to that of the third, and so on . . ." For Dashiell (38) the conditioned response is explicitly taken as "the

type and the elementary complete unit of all learning," but he is careful to state with equal explicitness that the situation is always more complex than the simple conditioned response paradigm makes it seem to be. To him, learning by insight is but learning to react to some highly specific element of a situation, "but that element is still a stimulus, however refined; and learning to react to it and nothing else is still a coupling of a motor response to a sensory signal." Herrick (75) likewise considers the conditioned response a fundamental paradigm, and Leary (105) believes that the conditioned response will suffice to explain all learning.

For Hollingworth (77) memorizing is the process of establishing the redintegrative effectiveness of subtle cues, the reduction of the requisite antecedent. The conditions favoring and retarding the rate of this reduction are well described, but one looks in vain for the principles or properties which render such reduction possible. *That* subtle cues come to stand for total patterns is clearly set forth; *how* they do, whether by frequency, by insight, or by some other factor, is not so clearly shown, although Hollingworth's treatment points toward a frequency doctrine, eked out by other factors of the same order, rather than toward insight.

In these accounts of learning, description of the completed event is mingled with interpretations of how that event came to its described completion. The former is a matter for experimental determination and is less directly open to dispute; the latter is more subject to interpretation in the light of theoretical considerations. However open to criticism the theoretical positions reviewed may seem to any particular reviewer to be, they are still significant and refreshing, and they amount to a rejuvenation of many problems of learning which were fast becoming sterile. This is achieved (A) by an attempt to envisage learning in a larger setting than that which it has usually been given in experimentation, and (B) by coördinating theories and facts of learning with a wider systematic structure.

Theories of Memory. Experimentalists have generally concerned themselves very little with the nature of memory. Fortified by the ready analogy of the phonograph record it has been possible to satisfy untrained inquirers while remaining undisturbed by the puzzling nature of the real problem. In some of the recent writing, however, the problem of memory has begun to receive critical treatment. Hollingworth (77) points out that memory is not the reproduction or resurrection of the past. A past event is remembered only by encountering a new event which can serve as a surrogate for the past

event. Such symbols typically operate unreported. Memory is thus not a matter of storing, but an act of restoration in which new events assume old patterns or relations. The changes which time brings to 'reproduced' content are largely the outcome of the loss of the cues which upon their first appearance were responses to the presented material. One wishes that Hollingworth had discussed the conditions which bring about the lapsing of the cues. Wheeler's (176) proposed theory has several points in common with Hollingworth's. Recall is not revival: "it is an effort on the part of the individual to experience a situation as he perceived it before." ". . . The difference between the responses to the earlier situation and the response to the situation partly repeated is the difference between original observation (perception) and memory." Memory is constructed not from residues but from present sensory stimuli. Remembering is thus an incomplete process of perceiving, initiated by some of the stimuli which conditioned the original experience. Behind this view there lies a theory of traces, as we shall presently see. For Köhler (98) reproduction is a matter not of association but of the operation of a total field, and depends upon the fate of 'traces.' Dashiell (38) treats memory as one with habit which depends on an appropriate stimulus for elicitation. One finds in Janet's (87) book a good critical survey of historical theories of memory and a well written exposition of the integral relation of memory and time. The former is a construct, peculiar to man, whereby he triumphs over time.

Bousfield (16) devotes a short book to a critique of mechanical theories of memory and to the presentation of a view of his own. The defects in a theory such as that of Semon's, *e.g.*, are: (A) one can obtain no direct evidence of a permanent change in cerebral cells which might form the foundation of memory; and (B) it assumes a definite molecular regrouping to correspond differentially to every possible impression, and such a definite and mechanical pattern can take no account of the diverse ways in which the same meaning can be reproduced, thus making the fatal mistake of leaving meaning entirely out of account. He holds that the essential thing in memory is the meanings, the ideas, not the words or the engrams. He regards the cerebral cells as organs for transmission rather than for storing. The latter must, however, be done somewhere and he presents an hypothesis which assumes: (A) a psychic structure as the basis of memory, which (B) involves an immaterial organization which serves to integrate a complex of sensory stimuli. How sensations are transmuted into ideas and retained as meanings is unknown, but that

the meanings are the important factor and that their retention involves a non-material organization is the major issue. By such an hypothesis, he holds, the specific areas of forgetting which occur in hysteria, following injuries and the like, are much more easily explained than by an engram view, since it is hard to see how an injury could pick out all the engrams deposited during the past week or about a given logical or grammatical focus. Bousfield's view has other points which will not be enumerated, but its chief characteristic is its attack upon mechanical theories of memory and its championing of retention as occurring in some immaterial form of organization which may be, he says, some form of modification of the ether. His criticisms are well put and are supported by contemporary work on cerebral function. His hypothesis will seem to most readers as somewhat less parsimonious with assumptions than one would like.

This attack upon the engram theory of memory leads to a review of the current statements about where and how 'memories' are stored. The first part of this section reviewed the descriptions of memory as an experienced mental event; this part will consider the 'traces' supposed to perdure from an experience to its remembering. The most frequently stated view is that experience leaves physical traces of some sort in the neurones of the cerebral cortex and that these traces are rearoused by stimuli similar to those of the original experience. Some of these theories are more sophisticated than others, of course. Learning is for Dashiell (38) a matter of synaptic change and retention a persistence of the altered synaptic condition. Bridges (17) subscribes to a neural view of memory. Learning is considered to be the formation of new bonds between neurones. Memory traces are not, however, effects in single cells but in the arrangement and relationship of cells. Traces are, for Herrick (75) structural alterations of brain tissue, but they are not passive. They are to be conceived as alterations of tensions which may react with one another at subconscious levels. This has some points in common with the *Gestalt* position that configurations are retained, not as specific traces, but as systems of stresses (Köhler, 98; Wheeler, 176). The effect of the eliciting stimulus is determined by its relation to the pattern of stresses functional at the time. The physiological stresses are not, of course, unchangeable impressions, but continually undergo change expressible in *Gestalt* laws. The two general tendencies are accentuating and leveling which have the common feature that both work toward the production of a 'better' *Gestalt*. (See Gibson, 55, and Hsiao, 81.) The points of view stated are typical of many

allusions to the mechanisms of retention. They have the common characteristic that all place the traces, which are assumed to be necessary, within the cortex.

The outstanding exception to this interpretation, aside from the theory of Bousfield, is to be found in Hollingworth's book. He deals with memory, as with all other mental events, without reference to the nervous system. All occur within the world of symbols, redintegratively operating, which is for him the world of mind. Memorial function takes its place within the continuum of natural events and no problem of mind-brain correlation exists.

The general willingness to place memorial residues within the brain depends for its validity upon the use made of the 'trace' concept. If it is frankly an attempt to guess where the assumed neural counterparts of remembered events are to be found, and nothing else, it has at least the status of an interesting conjecture. If, however, as is often done, either explicitly or implicitly, the neural traces are fallen back upon for explanation of the experimental facts of memory, logicity vanishes. One can not validly explain a sequence of remembered events by a set of traces, whether static effects or fluid stresses, which are not even known to exist and which are constructs made in the image of the memorial facts which they are then invoked to explain.

The Problem of Temporal Reference. This problem has long been known to the philosopher, but the experimentalist has glibly taken it for granted. It is noteworthy that reference to it appears occasionally in contemporary writing, although a systematic discussion is lacking. Events, as Hollingworth sees them, are given appropriate place in a system of report as they pass, and within this system they are symbolically placed according to their calendar notation. Likewise Bridges (17) writes: "the temporal reference of ideas is dependent upon their association with ideas of time such as calendar dates." For Wheeler, behavior in recall has only a logical reference to the past, it *means* the past. These statements take cognizance of the problem; one would like to see them elaborated. Janet's whole book furnishes material of importance for this problem.

Miscellaneous. Cathcart and Dawson (24) have demonstrated a phenomenon of the same class as Hollingworth's law of central tendency. When a subject is asked to remember an antecedent stimulus and to adjust a succeeding one to an equal intensity, *e.g.*, he does not go far enough toward the standard but accepts as equal a stimulus midway between his starting point and the standard. This

'persistence' factor they consider to be a general characteristic. Bloor (12) holds that forgetting does not mean that all traces have been lost. The trace of the forgotten fact endures subconsciously. Hosiasson (79) discusses forgetting and the subconscious. Claremont (30) recommends that memory be thought of after the analogy of the "button" of a searchlight beam, which can search within for records of consciousness. Masson-Oursel (116) discusses recall and retention as two techniques of memory, and Lindworsky (107) answers the critics of the theory of reproduction enunciated in his *Theoretischen Psychologie*. Rosenow (146) holds that recall in apparently complete posthypnotic amnesia is a matter of restoring the subject's original attitude of coöperation with the experimenter. Ewald (43) would add to memorial plasticity what he considers a second and independent factor, *viz.*, the size of complex which the learner is able to handle. The analogy between memory and heredity has been drawn again by MacBride (111) and by Crow (33).

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The writer has depended upon abstracts in English, French and German for the content of papers in languages other than the three mentioned.

NOTES AND NEWS

DR. HORACE B. ENGLISH has been appointed Professor of Psychology in the Ohio State University. Dr. Clarence Leuba, Lecturer in Psychology at Bryn Mawr, has been appointed Associate Professor of Psychology at Antioch College.

PROFESSOR MAX F. MEYER, of the University of Missouri, has been appointed research professor of psychology for one year, and has been relieved from all his teaching and administrative duties. His work will be concerned with methods of teaching the deaf and will be carried on with the clinical facilities of the Central Institute for the Deaf in St. Louis.

DR. A. T. POFFENBERGER, Columbia University, and Dr. Calvin P. Stone, Stanford University, have been elected representatives of the American Psychological Association on the Division of Anthropology and Psychology of the National Research Council. Dr. E. S. Robinson, Yale University, has been elected a member-at-large.

MR. C. W. MASON, M.A., Buffalo, 1930, and Mr. A. W. Melton, M.A., Yale, 1929, have been appointed Research Associates of the American Association of Museums. The former will serve as acting staff psychologist of the Buffalo Museum of Science, and the latter will serve similarly at the Pennsylvania Museum in Philadelphia. These appointments have been made in connection with a two-year research program of the American Association of Museums. A grant for this work was recently made by the Carnegie Corporation.

DR. HELEN THOMPSON WOOLEY, professor of education and director of the Child Development Institute of Teachers College, Columbia University, has presented her resignation to take effect on September 1.

AT Yale University, Dr. Mark A. May has been appointed director of the statistical bureau of the Institute of Human Relations, Professor Walter R. Miles and Dr. Catherine Cox Miles of Stanford University have been appointed research associates in psychiatry and psychology for one year.

DR. HULSEY CASON has accepted appointment as professor of psychology in the University of Wisconsin.

DR. PAUL L. BOYNTON has been appointed professor in the George Peabody College for Teachers.

THE PSYCHOLOGICAL BULLETIN

NINTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

REPORT OF THE TREASURER: JANUARY 10, 1928—AUGUST 8, 1930

Receipts:

Dues:

American members	\$8,970.00	
Foreign members	1,126.91	
		\$10,096.91

Grant from the Carnegie Endowment for International Peace
toward securing the attendance of foreign psychologists..... 5,000.00

Grant from the American Psychological Association for adminis-
trative expense 1,000.00

Interest on bank balances..... 381.77

Miscellaneous 15.55

Total receipts 16,494.23

Disbursements:

General administrative expenses:

Executive committee — Travelling and gen-
eral expenses \$187.18

Office expenses:

Treasurer 189.57

Secretary 44.42

Executive secretary 379.91

Foreign secretary 301.99

Program committee expense..... 445.87

Social arrangements committee expense... 654.98

Cost of Psychological Registers..... 387.71

\$2,591.63

Honoraria granted to foreign psychologists..... 4,702.43

Entertainment 3,157.40

Proceedings:

Editorial expense \$ 324.17

Printing 2,071.55

Binding 664.56

Mailing 322.43

Aquatints of Harkness Memorial Quad-
rangle 96.00

3,478.71

Return of dues received in error.....	\$50.00
Return to the Carnegie Endowment for International Peace of the unexpended balance of grant received...	297.57
Transfer of unexpended funds to the American Psycho- logical Association	2,216.49

Total disbursements \$16,494.23

R. S. WOODWORTH,

Treasurer

Audited and found correct: MARGARET FLOY WASHBURN

Audit certified: HASKINS & SELLS, New York, N. Y.

That this record of the Congress may be complete it is desirable to add the following data. The dues for foreign members were \$5, and the dues for American members \$10. The dues included the Proceedings. Foreign members were guests of the Congress for both lodging and board. American members paid \$1 a night for rooms in the Yale dormitories, and had cafeteria rates for meals in the Yale dining hall. Foreign members were guests at the banquet, and American members paid \$3 each for tickets.

It is not practicable to include here expenses of entertainment for foreign members before and after the Congress by institutions other than Yale University. There was entertainment provided before the Congress at Princeton University and Columbia University, and after the Congress at Smith College, Clark University, Harvard University, Wellesley College, and by Professor Warren at Woods Hole. Yale University in its entertainment incurred a deficit of \$3,006.10, for which it was reimbursed by the Congress. A brief summary of accounts of Yale University for entertainment follows:

YALE UNIVERSITY	
<i>Receipts</i>	
Room-rent	\$2,915.00
Dining hall	3,610.66
Banquet tickets	1,320.00
Miscellaneous	55.67
Voted from general funds by the Congress to cover deficit at Yale.....	3,006.10
	\$10,907.43
<i>Expenditures</i>	
Dining hall	\$4,898.15
Banquet	1,662.00
Rooms and miscellaneous.....	4,347.28
	\$10,907.43

E. G. BORING, Secretary

PSYCHOLOGY IN INDUSTRY¹

BY MORRIS S. VITELES

University of Pennsylvania

The literature on industrial psychology appearing before 1927 has been reviewed by the writer in two earlier publications (454, 455). In the present article are reviewed publications in this field appearing between January 1, 1927, and June 30, 1930, inclusive. As in the earlier reviews, the policy has been followed of including only books devoted primarily to industrial psychology and articles on the industrial applications of psychology appearing in the technical journals. This arbitrary selection of material has probably resulted in the omission of worthy publications which have appeared in other periodicals.

As in the case of earlier reviews, a special effort has been made to cover completely the German and French literature. There may be a few omissions by reason of the difficulty in obtaining a number of foreign books and back numbers of foreign publications, but the writer believes that these are few in number.

I. THE SCOPE OF INDUSTRIAL PSYCHOLOGY

Recent years have witnessed the appearance of a number of books covering the entire field of industrial psychology and of numerous articles defining its scope and psychological foundations, surveying its general accomplishments, etc. In view of the number of such publications, it seems well to group them in an introductory section of this review before proceeding to a discussion of investigations in the specific fields of vocational selection, merchandising, and of other industrial applications.

Among the major contributions of this sort is a small volume, edited by Myers (287), giving an excellent resumé of problems, methods, and accomplishments in the field of industrial psychology. A chapter by Myers, showing the place of psychology in promoting the welfare of the individual and in increasing production in industry

¹ The writer is indebted to Kinsley R. Smith, a graduate student at the University of Pennsylvania, who assisted in assembling the material for this review.

is followed by chapters on work and environment, work and rest, measurement of vocational aptitude, etc., by investigators in each of these fields. Although reporting almost exclusively findings in English studies, this volume gives a clear picture of the scope of industrial psychology, as viewed by European psychologists, and a broad survey of specific accomplishments.

A more comprehensive treatment of the theoretical foundations, the scope, achievements, etc., of industrial psychology is embodied in a large volume by Giese (158) including a very detailed account of German and other studies on selection, improvement of working methods and conditions, training, prevention of accidents, and of related applications of psychology in industrial life. Of particular interest is the discussion of the contributions to applied psychology, of psychiatry, and of psychological theories,—of the viewpoints of Adler and Freud, as well as of *Gestalt* psychology. It is unfortunate that this valuable book, enriched by many illustrations, should suffer from the somewhat unorganized presentation of material so characteristic of the author, and from an absence of an index and of a bibliography. A somewhat condensed survey of the same ground is included in other volumes by the same prolific author (159, 160, 161). An interesting item in one of these (159) is a calendar showing the outstanding dates in the development of industrial psychology. Freund (140) contributes a sketchy but well written discussion, for the casual reader, of the high lights of industrial psychology, including vocational selection, training, advertising, etc. A text book on industrial psychology is provided by Moede (278).

Among the books not devoted exclusively to industrial psychology, but including a great deal of material from this field, is a volume by Devinat (104) embodying an informative survey of developments in scientific management, and a description of contributions from industrial psychology. This book reflects the association in Europe of two fields of activity exploited by distinct groups in this country. This association, appearing in all major European publications, has been discussed elsewhere by the writer,² and needs no further discussion here. A valuable directory of institutions for the application of psychology and of physiology, and quite complete German and French bibliographies are found in this helpful volume. Equally valuable to the industrial psychologist, although oriented from another point of view, is Poffenberger's (314) comprehensive survey of applied psychology. Findings and methods employed in selection, in

²Viteles, M. S., *Psychology in Industry*. *Psych. Bull.*, 1926, 23, pages 657-60.

the study and amelioration of fatigue, in the consideration of methods of work, etc., are summarized in this well organized treatise. Less satisfactory are the chapters on the industrial applications of psychology in a book by Moss (283).

The psychiatric point of view in industry finds its expression in a volume by Anderson (11) given over largely to a description of accomplishments in the development of standardized interviewing techniques, the treatment of problem cases, the development of psychological tests, etc., for use in a department store. The volume is a record of definite accomplishment through the application of the "clinical method," in contrast to the "statistical method," in the selection of workers, and in the treatment of individual maladjustment. It reflects, however, the abnormal orientation so characteristic of psychiatry, and the psychiatrist's failure to employ objective methods in the evaluation of specific personnel procedures, as well as his dependence upon the testing techniques developed by psychologists.

Among other volumes of general interest in considering the broader aspects of industrial psychology is a compact and readable summary, by Cathcart (85), of work in England and in the United States, including a bibliography, and books by Chellew (87), Meakin (268), and Nolte (296). An extremely popular treatment of the value of psychology to industry, written in staccato style, punctuated by somewhat lurid and uncritical references to experimental studies, is contributed by Laird (234).

Of historical interest is a new edition of Münsterberg's (286) pioneer work on industrial psychology, including as its only change a supplementary and more recent biography.

The proceedings of various international and other conferences on industrial psychology serve as general guides to recent developments in content, method and changing viewpoints in the application of psychology in promoting human welfare and efficiency in industry. Among these are reports of the International Congresses of Psychotechnique (12, 13), and proceedings of meetings of the International Industrial Relations Association (36, 134).

Among shorter publications on the scope, methods, objectives and theoretical foundations of industrial psychology is a discussion by Lippman (248, 250) of the principles of industrial psychology in the broader study of all the factors affecting productive work by those interested in what the author designates as the "science of work" (*Arbeitswissenschaft*). This relationship is likewise dis-

cussed by Plaut (313). In another article by Lippman (249) is included a survey of human problems in production, and proposals for the systematic examination of such questions as human *vs.* machine factors in production in diverse industries, the importance, among human factors, of individual aptitudes, etc.

The scope of influence upon industrial thought and practice of industrial psychology as science, point of view, and method is outlined by Bingham (60) in a stimulating article incorporating a brief description of agencies in the United States furthering industrial psychology. Methods and points of view of both psychology and psychiatry are contrasted with those of the American exponents of scientific management, by the same author, in a later article (61), in which he briefly summarizes and optimistically evaluates accomplishments in the elimination of fatigue, in the scientific study of employee attitudes, of effects of physical and of social environment. The outlook and accomplishments of industrial psychology are also briefly reviewed by Tead (426).

A somewhat novel and realistic attitude is found in a discussion by Arend (37) of the importance of industrial psychology for European countries in meeting the competition of standardized, high speed American industry. In a somewhat popularly written article on the work of the National Institute of Industrial Psychology (14) is found a further indication of the scope of application of psychology in mending industrial ills. The general social significance of industrial psychology is discussed by Gagg (145).

Theoretical foundations are the chief concern of other shorter publications. Moede (279) outlines the theoretical foundations of industrial psychology. The essential concept of this science, according to the author, is production. The significance of individual genetic and social factors affecting human production must be analyzed, the objective of such an analysis being the formulation of general and specific laws covering human production. Examples include the principle of individual differences, the effect of age upon ability and production, etc.

Viteles (449) stresses the objectivity of the psychological approach in industry, its dependence upon laws of human behavior established in laboratory investigations, its orientation in terms of normal behavior in contrasting the viewpoints of psychology and psychiatry in industry. Brief references to investigations in accident prevention, training, fatigue, etc., are included in this article. The general significance and scope of psychiatry in industry is discussed

by Elkind (125). The definition of psychiatry is made broad enough to include much in the way of method, viewpoints, and techniques described as characteristic of the psychological approach by Viteles (449). Citation of evidence includes, in the main, the work of psychologists, as well as that of Anderson, one of the few active exponents of the psychiatric approach in American industry. Anderson (8) and Smith (376) contribute shorter articles on psychiatry in industry.

In a report of an English symposium (15) on the attitude of employees toward psychological investigations, there are outlined in a general way, the principles underlying the application of psychology in industry, and the distinction between the humanistic character of the psychological approach to problems of industrial efficiency as contrasted with the mechanical outlook of the efficiency engineer.

An article by Kornhauser (217) is given over to a comparison of points of view, organization, and accomplishments of industrial psychology in England, Germany, and the United States. The emphasis in this country on selection is contrasted with European interest in methods and conditions of work. Many facts are combined with judicious observations in this thumbnail sketch of the present status of industrial psychology.

The status of industrial psychology in the United States is appraised by Bingham (62) in an article describing the investigations which have been conducted, and embodying a recommendation for the establishment of a National Institute of Industrial Psychology in the United States to effect a wholesome interaction between the science and industry.

Heller (178) describes the development of industrial psychology in Switzerland, growing out of the early work of Suter, and leading to the establishment of a Swiss Foundation of Industrial Psychology. Vocational selection, the organization of training on a scientific basis, study of methods of work, etc., are included among the activities of Swiss industrial psychologists. A statement on savings effected and a comparison with work in the United States are included in this informative review.

The manifold applications and accomplishments of psychology in industry in England are described in an article on the work of the National Institute of Industrial Psychology, by Myers (288), in a concise survey by Fryer (142), in the annual reports of the National Institute of Industrial Psychology (17, 18, 19, 20), and in annual

reports of the Industrial Health Research Board of Great Britain (33, 34, 35).

A brief description of the present status of industrial psychology in Spain is included in an article in a British journal (16).

A brief description of developments in vocational selection and of investigations of fatigue, effects of temperature, etc., in Japan is incorporated in an article by Yenjiro (478). It is interesting to note that although at the start American influences predominated, there has gradually been a shift toward an acceptance of German concepts and techniques. Agencies for the application of psychology in industry include governmental bureaus, semi-private agencies, etc.

Brief reports on international congresses on industrial psychology are found in American and foreign journals (190, 339).

As a contribution to the history and to the description of the present status of industrial psychology, Baumgarten (48) presents a calendar of dates of the initiation of projects and of publications in the field of applied psychology—with particular reference to industrial applications—as a means of settling to the satisfaction of the author the problems of priority over which the author, among other Germans, has been greatly disturbed.

II. VOCATIONAL SELECTION

1. General

References to the application of psychology in the selection of workers, in some cases taking the form of detailed descriptions of tests, are found in many of the general texts and articles discussed above, under *Scope of Industrial Psychology*. In addition, during the period covered in this review, there have appeared a number of major contributions dealing exclusively or primarily with vocational selection. Among these is a comprehensive survey by Baumgarten (49). The first part of this book is devoted to a dissertation on the foundations of scientific selection, including a discussion of individual differences, the nature and origin of individual aptitudes, interests, etc. This is followed by chapters on job analysis, classification of occupations and principles and techniques in the development of selection tests. The major portion of the book is given over to a detailed review of procedures which have been developed in Europe and in the United States for selecting workers in diverse occupations. In the case of each, the author includes a description of the tests and of results which have been obtained. A bibliography of 1,200 titles completes this imposing work. The volume is inval-

able by reason of the richness of its contents, in spite of occasional errors with respect to individual—particularly American investigations—and the expression of bias rather than of judgment in distinguishing among investigations with respect to practical significance and scientific value.

A description of tests used in selection for clerical and mechanical occupations is included by O'Connor (298) in a volume teeming with aesthetic judgments and with what are virtually metaphysical speculations on the nature and origin of aptitudes and of individual differences in aptitudes. The book is a curious combination of carefully presented data representing real accomplishment in the selection of workers for a few specific occupations, by one essentially unversed in the refinements of validation, and of unwarranted assumptions and conclusions in the fields of biology and psychology. Not the least serious of the metaphysical excursions is a series of vocational prescriptions for young men and women based on tests and standards developed in specific industrial situations.

Among other publications primarily devoted to vocational selection is a second edition of Laird's book on the psychology of selecting men (235), and a consideration by Schindler (351) of the significance of vocational selection from the viewpoints of scientific management, of social and political welfare, which includes a description and discussion of private and governmental agencies employed in this work.

Among volumes not exclusively devoted to the use of psychological tests and other scientific selection procedures in industry, but having an important bearing from the viewpoint of industrial application, is a book by Hollingworth (189) combining material from two earlier volumes on the use of the letter of application, the estimate of character from photographs, phrenology, the interview, psychological tests in the measurement of mental competency, the significance of interest, intelligence, etc., and including an appendix of laboratory exercises and a bibliography. The chief significance of the volume is *historical*, in the sense that it carries the reader back to earlier investigations of subjective and objective methods of character analysis for vocational purposes, and *critical* in its emphasis on the practical usefulness of subjective methods. The failure to include reference to very important recent work in the measurement of vocational aptitude seriously limits the usefulness of this book, as interesting as it may be as a reference work, and as

a link with the beginnings of vocational psychology by one who early gave his attention to its problems.

Somewhat in contrast to Hollingworth's book, from the point of view of immediate serviceability to the industrial psychologist interested in the development of selection tests, is Hull's (196) volume on aptitude testing. The clear and highly critical discussion of the nature of aptitudes, the comprehensive treatment of procedures, cautions, and statistical methods in the construction, calibration, and validation of aptitude tests, and the illustrative material make it an invaluable aid in the practice of industrial psychology, and in the appreciation of the theoretical foundations and techniques of aptitude testing. Among other publications which, although in no sense limited to vocational selection, are of distinct interest to those dealing with vocational selection, is a volume devoted to the history, theory and description of mental tests by Freeman (139). The critical attitude of the author makes it helpful in understanding the uses and limitations of tests. In the same way, a discussion by Wells (466) of the use of mental tests in clinical practice embodies comments on testing procedures and the limitations of psychometric measures which should be known to the industrial psychologist. A manual by Bronner, Healy, Lowe and Shimberg (76), containing descriptions of procedures and norms for many standard tests, descriptions of tests by Drever and Collins (113), and a discussion of the use of tests in an experiment in vocational guidance by Earle, Milner *et al.* (119), represent useful additions to the library of the industrial psychologist.

A number of shorter articles and manuscripts are devoted to underlying *theory in test construction and standardization* and to *administrative problems* in the use of tests for the selection of workers. Rupp (340) discusses the theory and practice of psychological testing, stressing the importance of exact objective units of measurement, of qualitative observations and of the consideration of the influence of practice and conditions of administration, etc., in the measurement of vocational aptitude. The complexity of the selection and guidance problem is pointed out by Wallon (457), who refers to studies showing the significance of such factors as interest, the influence of heredity and environment, etc. There is included a brief discussion of the possible application of psycho-analytical techniques and viewpoints—particularly of Adler—in discovering the obscure tendencies which motivate the individual in occupational life. Marbe (263) distinguishes between “psychological aptitude”,

which appears in a test performance, and "vocational aptitude", based on the former, but reflecting training.

Hanna (174) sketchily outlines the steps to be followed in the standardization and validation of tests of vocational aptitude. Kitson (209) pleads for a comparison of scores on a single test of workers in diverse occupations, and for the comparison of scores made by vocationally selected and unselected groups as necessary steps in the development of tests, if they are to be useful in the selection or guidance of individuals with mediocre vocational aptitude, as well as of the highly superior. The viewpoint, whatever merit it may have, is, of course, less applicable in selection, in which interest may legitimately center on the extremes of a distribution, than in guidance. The steps to be taken in the calibration and validation of selection tests are also discussed by Lahy (230), who, unfortunately, uses as an illustration the validation of a test of bi-manual coördination on a total of five "Good" and five "Poor" operators.

Stern (409, 410) stresses the global character of personality in discussing what he has termed "personalistic" psychology. Of special significance, with respect to the construction and use of tests is his criticism of the analysis of human behavior into so-called functional elements, and his insistence upon the development of selection tests resembling the entire activity in which the individual is to engage. Juhasz (203) follows Giese and Stern (409, 410) in a demonstration of the importance of viewing the total personality of the individual, in contrast to a mosaic of independent elements, in the prediction of vocational attainments. The approach reflects the influences of a *gestalt* viewpoint in psychology as applied in the measurement of individual differences.

The relation of vocational testing to Spearman's two factor theory is discussed by Earle (120).

Objections to the use of psychological tests in selecting workers are voiced by Sotonin (385) in an article describing the work of a Russian institute for the application of psychology in industry. Among the most interesting of these objections—if not the most reasonable—is that based on the view that occupations seldom require the development of specialized abilities above the level attainable by the average individual. This viewpoint parallels the very general assumption of Kitson (209) that 50 per cent of the population will succeed with a 50 percentile degree of success in 50 per cent of occupations. Other theoretical considerations pre-

sented as arguments against an undue insistence upon careful selection include the greater significance of general, as compared with special ability, the infrequent occurrence of specific defects, etc. The increased mechanization of industry is cited by Lippman (251) as a factor which will result in a decreased need for highly specialized selection tests.

Problems in *statistical treatment* are the burden of other articles of interest from the viewpoint of theory in vocational selection. The importance of recognizing psychological dangers and limitations in statistical methods is discussed by Myers (289), who refers specifically to the use of certain of these measures (correlation and reliability) in connection with the validation of vocational tests. Kitson (209) objects to undue emphasis on correlation techniques in the validation of tests in vocational selection and guidance. Critical comments on the calculation of correlations are included in an article by Rupp (341). A comprehensive discussion of frequency curves, with particular reference to the significance of each of six types in the calibration and validation of tests as well as in other psychological research in industry, is contributed by the same author (342). Lazarsfeld (242) discusses the normal distribution curve from the viewpoint of tests used in the measurement of production. Frequency curves in industry are also discussed by Lehmann (243).

A method of treating test results to permit the combination and scoring of the two variables speed and accuracy is suggested by Lahy (231). Strong (416) compares formulæ used in arriving at weights for scoring an interest inventory.

A problem of *administration* is touched upon by Dixon (107), who makes the discovery that psychologists are adopting the viewpoint of psychiatrists in insisting upon more than test scores as employment criteria. The author appears to be ignorant of the technical literature on the use of tests, and of the historical background and viewpoints of clinical psychology. The limitation of test scores and the importance of a supplemental psychiatric analysis are also discussed by Ray (335) and by Hubbell (195). The latter rates aptitude as having a weight of only 25 per cent in vocational proficiency; attitude, training, and supervision accounting for the remaining 75 per cent. On the basis of this analysis, the relative importance of selection tests, as compared with the psychiatric interview, is discussed. There is the failure, characteristic of psychiatry, to present objective data to substantiate the generalizations on the

administrative limitation of testing techniques, and on the validation of other methods. The relation between the psychological testing laboratory and the employment office in the administration of tests is discussed by O'Connor (299).

Findings from *follow-up* studies and general problems of *validation* are considered in a number of articles. Roloff (331) reports on a very thorough validation of tests used in the selection of apprentices for the metal trades. Twenty-eight tests were applied to 34 apprentices divided into three groups, carefully selected by foremen with respect to character of work, from among a great number of employees. The contribution of each test, the comparative value of objective and subjective judgments, the relation between the length of each test and test validity, the relation between the number of tests and the validity of the battery, are carefully studied and judicious conclusions drawn therefrom. This study represents one of the most careful evaluations in a field in which many hasty conclusions have been drawn. Bramesfeld and Taubeneck (70) also report an unusually thorough study of a large battery of tests, of the analytic type, used in the selection of apprentices. There are discovered marked differences in reliability among the tests—the so-called sensory tests showing, in general, lower reliability than tests of motor ability and of general intelligence. This is also true in the case of validity. This follow-up of test results is considerably weakened by the small number of subjects—only 12 in the case of some tests—and by a highly unsatisfactory criterion, but it is outstanding among investigations in this field in recognizing the significance of a careful study of reliability and validity and of follow-up in the development and use of selection tests. The validation of a battery of vocational tests is also discussed by Biegeleisen (59).

Ruffer (337) reports satisfactory correlation between pre-employment test scores of 104 workers and curves of daily production of lamps. The objectivity of the criterion—a characteristic which still appears all too infrequently in selection studies—adds to the significance of his findings. In a comparison of pre-employment test scores with the work records of 131 apprentices who had completed a four year apprenticeship, Heilandt (176) finds a confirmation of the validity of the tests. The criterion is a complex of ratings by supervisors, final examination grades in an apprenticeship school, and scores on a work examination.

Skaggs's (370) comments on validity, in a discussion of prevailing concepts in mental tests, applying to industrial testing, in which

subjective criteria have been so frequently employed. Bramesfeld (71) considers the difficulties experienced by foremen and supervisors in making judgments on the accomplishments of working personnel.

Provisions for the application of selection tests are reviewed in a few articles. The plan of organization of a testing service with decentralized branches is described by Couvé (91). Payne (304) describes tests used in the selection of workers, and refers to agencies engaged in developing and giving tests in a highly uncritical discussion of how workers are tested in Germany. The extent to which vocational selection tests are used in Germany and Holland is briefly reviewed by Prak (322). The status of vocational selection in Hungary is summarized by Sandor (347). Heydt (197) contributes a brief description of the scope of scientific selection by the State Railways of Germany, in connection with which there are 27 centers applying tests for aptitude in many types of work. The development of scientific methods of job analysis and selection under the auspices of the United States Civil Service Commission is sketched by O'Rourke (302) in an article including samples of a few tests. Significant from the viewpoint of the broader application of tests is the program recommended by the author for the coöperation of industry, schools, and the Federal personnel service in making available the results of standardized tests, developed by coöperative research, for purposes of guidance of the individual, as well as for selection in diverse industries.

General problems in the measurement of *temperamental* or *personality* traits continue to occupy the attention of industrial psychologists. Bathhurst (47) discusses the importance of temperamental traits in vocational adjustment and furnishes specifications of social characteristics for various jobs in the form of scores on a Diagnostic Temperament Test. In contrast to his buoyant confidence in the scores on temperamental tests is Baumgarten's (50, 51) recommendation for the exercise of caution in the judgment of character on the basis of tests. Couvé (92) also warns against the uncritical use of so-called tests of character in vocational selection, and points to the possibility of making observations of such traits as endurance, will, etc., in the course of performance on aptitude tests. Reference is made by Schmitt (352) to the existence of character types in industry. A disregard of fundamental principles in character measurement is reflected in the publication of a presumably universal test of honesty (21).

Earlier experiments and conclusions on the relationship between *interest* and *ability* are summarized by Fryer (143), who draws the conclusion that vocational interest is not a significant criterion of vocational aptitude. Kitson (210) asks teachers and nurses to rate themselves on interest in their profession, on a rating scale embodying a highly imaginative criterion. From an examination of interest of these workers in their occupation he draws highly questionable conclusions concerning the significance of interest in vocational adjustment. Further experimental results on the place of interest in vocational adjustment are presented by Strong (417).

Among miscellaneous specific problems relating to the use of tests in vocational selection are reports on investigations of the relation of age to test performance. Ehinger (122) reports a preliminary study of the effect of age on performance on 5 tests involving the examination of 181 workers, 22 to 50 years of age, with from 7 to 31 years of experience in a factory. The findings reveal superior test performance on the part of younger employes, a superiority which also appears in production. A similar investigation by Weiss (465) in the case of railroad employes leads to similar findings. The latter expresses the opinion that (1) a diminution of ability to adapt oneself to a new situation, and (2) a deterioration of intellect explain the decrease in efficiency with age in certain of the tests employed in this study. The findings of Glasel (163) with respect to the general trend and to the variation among individual tests confirm those of Weiss (465).

2. *The Measurement of Motor Capacity in Industry*

The general problem in vocational selection which appears to have received particular consideration during the period covered by this review is that of the measurement of what has been designated with the various names of *motor ability*, *skill*, *manual aptitude*, *motor capacity*, etc. A number of first rate contributions on the nature of motor capacity (to adopt one of the names) and on measurement techniques have appeared. In view of the very general significance of this problem, it seems well to discuss these contributions together, although findings on the prediction of proficiency in specific occupations are to be found in a number of these studies. A brief reference to the latter will be made under appropriate captions below.

The discussion of this problem, as it has been treated in the literature, entails a consideration of the controversy on the relative

suitability of the "analogous" and "analytic" types of test, and leads, in a few instances, to reference to what are virtually experiments on learning.

The ambiguity of terminology in this field is referred to by Pear (306, 307, 308), who defines *skill* as an integration of well adjusted performance—a pattern of performance characterized by complexity, integration, and adaptability to changing situations, the aptitude for which is based upon well coördinated reflexes, instincts suitable to the task, adaptive habits, and the power, or maybe powers, of patterning. In this sense, there may be mental, executive, social, as well as hand skills. Pear's discussion is largely centered around the latter. Distinctions are drawn among *capacity*, based on inherent mechanism, *ability*, representing a level of functioning determined largely by practice, and *habit*, representing efficiency in a specific operation. The importance of an analytic study of the kinds of skills required by industry is pointed out, and a tentative classification of industrial skills is offered. Pear's treatment of this basic problem—his integration of material from laboratory investigations on habit formation, the transfer of practice, the measurement of differences in so-called psycho-motor traits, and facts of an economic order—is thought provoking. At the same time, although the importance of his contribution is undeniable, it is questionable whether his treatment satisfactorily clears up the ambiguity of terminology and the befuddled thinking on this subject about which he complains.

As Pear points out, there is need for systematic experimental investigation of concepts and theories on the nature and measurement of the so-called motor capacities as one factor in certain kinds of skill. Such a systematic experimental approach is employed in a unique study by Fairchild (128), who adapts the Gilbreth motion study technique to the study of skill (as defined by Pear) of 75 workmen employed on 11 types of machines in 4 plants. The findings (embodying case studies of individual workers to be reported in detail in a later article) indicate that where skill exists, and is given play to a considerable degree, it tends to become a first and greatest source of satisfaction to the worker, and that specialization of work may certainly be associated with lessened skill in workers. A summary of viewpoints on the nature of skill, and a bibliography on skill and mechanical ability are included in this excellent article, the implications of which cannot be neglected by those interested in the measurement of motor capacities.

Pear's point of view is also reflected in an investigation of "manual dexterity" by the National Institute of Industrial Psychology, reported by Earle, Gaw, and others (121). Although the subjects are children between the ages of 12 and 17, 1,000 in number, the findings and conclusions are significant from the viewpoint of vocational selection. The word dexterity is used to mean precision in the control of movement, or speed in the repetition of a movement, or both in combination, the dexterities representing the habit forming capacity of the individual in respect to bodily adjustments, as distinguished from skill, which involves their effective application in some particular task. The investigators distinguish between "manual dexterity" and the general factor of "motor ability" which has been assumed to underlie bodily activity. The investigators recognize as an important aspect of the study the question as to whether the skill of an individual at any given step of development is a reliable measure of his habit forming capacity. The report embodies an analysis of the tests used, a wealth of statistical and other data, and conservative conclusions with respect to sex differences, reliability, age differences, differences due to training, etc. The study of the inter-correlations brings out a high degree of independence of abilities, measured by them, when Spearman's tetrad difference criterion is applied to the table of correlations, the factors determining success in each appearing to be specific to the test situation. However, the fact that tests may be grouped to show higher correlation between themselves than with those other groups may be taken to signify the presence, rather than the absence, of group factors. The weakest conclusion, although stated in very tentative form, is one on the possible usefulness of certain tests of dexterity in selecting best trainees for an occupation, based on a correlation of test results with the very unsatisfactory criterion of teachers' estimates of proficiency in manual occupations.

Farmer (129) reviews the positions of Perrin and Muscio and discusses the intercorrelation of a group of "aestheto-kinetic coördination tests". The author points out that when grouped, a common factor seems to appear, and concludes that further experimentation may definitely reveal group factors in sensory-motor activities. However, he agrees with Muscio and others that vocational tests should be specific to the situation. In another article, the same author (130) discusses the results of 1,342 subjects, divided into 8 groups, on choice reaction tests, involving 30 stimuli. Curves showing average time of each stimulus for each group are parallel,

as is also the case, according to the author, with results of psychological tests of a motor order used in industry. It must be observed that the averaging of scores of large numbers obscures the individual differences in curves to which other investigators, as, for example, Robert (329) attach great significance.

A resumé of the more important work on the measurement of motor capacities is contributed by Seashore (368), who also describes an investigation in which 8 tests were applied to 50 University students. Data on reliability, on the effects of practice on the motor activities, correlations among the tests, etc., is given. The results confirm those of other studies in showing a consistent, although low, positive correlation among most motor tests. The author suggests that in job selection, a sampling of serial performances of the same general motor-skill coördinations involved, would be the most likely device for predicting a practical "motor-skill". The Stanford Motor Skill Unit, a battery of tests for experimental work in this field, is described in other articles (369) embodying additional findings with respect to these tests.

Recommendations for the use of work curves on simple operations, in preference to tests of the "analytic" type, in measuring motor capacity and in predicting proficiency in manual operations are found in the publications of other investigators. European psychologists seem to be divided into two distinct camps on this question—with Moede and Piorkowski in Germany carrying the banner for the "analytic" tests, and with Poppelreuter and Giese waging the war for the work curve. In a statement of his viewpoint Poppelreuter (318) criticises the prediction of vocational proficiency on the basis of a few tests of short duration, and insists that a prognosis should be made only after an examination lasting for more than an hour and involving a performance resembling closely that of the work itself. He cites examples of tests, involving in one case simple and monotonous manipulations in placing balls into apertures; in another case, attention in finding boxes marked with characteristic signs, etc. Work or practice curves, recorded automatically, are analyzed to show errors in diagnosis and prediction of proficiency on the basis of performances of short duration.

Experimental investigations of the practice curve as a selection device have, in practically every instance, involved a measurement of motor capacity and a prediction of motor proficiency. Learning factors have naturally been considered in this connection. So, for example, Gemelli (156) presents a critical survey of the concept of

manual ability and arrives, on the basis of an experimental analysis, at a definition of "manual ability" in terms of a fixed form, or *gestalt* of movements through practice and associated with the adaption of each movement to a specified end. His experiments lead him to favor the conclusion that the diagnosis of manual ability, as, for example, in the case of applicants for apprenticeship, should be based upon an examination of work curves, rather than upon "analytic" tests of specific traits. Incidentally, he suggests the desirability of using work curves obtained during the early period of training in preference to those obtained prior to employment, on performance simulating the occupation. In another article by the same author (157) is a brief review of the literature and a description of an experiment involving spinners and operators of lasting machines in a shoe factory, the results of which lead to similar conclusions. The findings agree with those of Argelander (38) on the relation between performance during the first part of a working period, and the effect of practice in decreasing individual differences in production, etc. The greater effectiveness of practice on the work itself, as compared with practice on an analogous task, is suggested by the results of a study by Langdon and Yates (239), which indicate that in manual dexterity and visual discrimination the effect of training is specific rather than general.

A critical evaluation of the work sample method recommended by Poppelreuter (318), partly on the basis of an experimental study, is included in an article by Winckler (469). The author disagrees with the insistence of the former upon complete liberty to the subject in carrying on his work. He also finds in the work sample situations incentives similar in part to those found in the work test. The author points to the desirability of supplementing work samples with tests measuring traits not subject to examination by work samples. He also recommends qualitative analysis in the case of both work samples and tests. The use of work samples is also urged by Baurmann (52), following an experimental investigation of differences in rate of work on a drill press.

Sollier and Drabs (383) employ reflex movement of defense in examining the influence of training on success with a simple motor act. They confirm the findings of Argelander (38) and others concerning the failure of those starting with a poor performance to reach the level of the best. The investigation is oriented from the point of view of selecting workers. Another investigation by the same authors (384) leads to the conclusions, with respect to

absolute speed, that the highest limit of performance, when the plateau is reached, can be considered as an index of perfectability; that the order of subjects established at the beginning of practice hardly varies, etc. It is found, however, that the same rules do not apply when precision is combined with speed. The results suggest that, contrary to common opinion, trainability in precision does not depend upon perfectability in speed. However, the conclusions, obtained on 10 subjects with a somewhat limited time for practice, may be considered as tentative. Robert (329) studies the practice curves of two groups of ten apprentices in 5 operations, including bi-manual coördination, perforation of a paper band, etc. His findings lead him to stress the importance of studying the work curve of the individual, showing the nature of his trainability and adaptability, in vocational selection. Ehinger (123) reports an important investigation on the effect of manual work upon performance in 4 tests of manual ability. The subjects include (I) 25 students in a pre-apprentice class, tested during the first week of apprenticeship, and again after a lapse of 5 months; (II) 88 workers, aged 14 to 30 years, tested prior to employment and after 3 months to a year subsequent to employment; (III) a control group of 54 pupils of the same age as the apprentices, and (IV) adult workers employed in a factory for at least 7 years, examined twice at 3 month intervals. The author finds that the second test performance is in every instance superior to the first, but that the children in the control group show less improvement than the apprentices, although retested after a shorter interval. There is relatively more improvement in the case of those testing low on the first performance than in the case of those testing high. Although a control group of adults would have been desirable, these and other findings in the study are of significance in view of the meagerness of direct experimental evidence on the effect of practice on a manual occupation upon performance on tests of motor capacity. Wallon (458) Kellner (206), and Erschowitz (127) also contribute discussions on the type of tests to be used in the measurement of manual ability.

The fact that there is no universal dissatisfaction with the "analytic" type of test for the measurement of motor capacity is reflected by the publication in recent years of a number of articles describing such tests, and furnishing more or less statistical data on their validation. The Minnesota Mechanical Aptitude Tests are described and discussed in an article by Anderson (6). Stoy (414, 415) describes a series of tests which are compared with instructors'

ratings and with independent ratings on a group of mechanical drawings. The author draws conservative conclusions on the possible usefulness of these tests in measuring traits involved in mechanical drawing, and in other forms of motor skill. MacQuarrie (261) describes a paper and pencil test of mechanical ability, and offers very inadequate evidence of its validity. Evidence of similar value on this test is included in an article by Stein (408). The "wiggly block" as a measure of mechanical aptitude is discussed by Keane and O'Connor (205) whose figures show that the test distinguishes between mechanics and draftsmen as compared with an unselected group. Schultz (361) describes a test for motor capacity involving bi-manual coördination. A detailed analysis of coördination and descriptions of procedures for its test measurement, are included in a monograph by Meistring (271). A scale of tests for measuring motor capacities involved in gymnastic activities is described by Brace (68). A volume by Schulte (358) includes material on the measurement of motor aptitude for athletic games.

3. *Specific Occupational Tests*

A. Tests for Skilled and Semi-skilled Workers.

In the investigations of aptitude for factory work Europe, and particularly Germany, still continues in the lead. A number of the studies to which reference has been made in the discussion of books and articles of interest from the standpoint of general problems in selection embody findings referring specifically to the selection of skilled and semi-skilled workers. Among these are the publications by O'Connor (298), Roloff (331), Kellner (206), Bramesfeld and Taubeneck (70), Ruffer (337), Heilandt (176), Fairchild (128), Gemelli (157), Baumann (52), Poppelreuter (318), Robert (329), Erschowitz (127), Keane and O'Connor (205).

Additional contributions on selection for occupations requiring skilled and semi-skilled workers include a number dealing specifically with metal trade apprentices. In an article by Kellner (207), is found a brief history and description of procedures used in the selection of apprentices in the metal trades of Berlin. The author employs an ingenuous procedure to show that, on the whole, suitably selected apprentices can be expected to be 15 per cent more efficient than those haphazardly selected, at a gross saving of three hundred marks over a four year apprenticeship period. Lahy (232) follows a verbose discussion of changes in the aptitudes of apprentices imposed by the introduction of machines with a brief description

of an investigation involving a comparison of the test scores of 53 apprentices with the accomplishments in academic subjects and in the work shops of an apprentice school. Anderieth and Rupp (41) contribute a detailed description of procedures employed in the administration of group and individual tests in the selection of machinist apprentices for the work shops of the Austrian Railways. In spite of the absence of correlation between test results and plant production, satisfaction is expressed with the tests by reason of improvement in related school work and because of the better impression made upon foremen by those tested prior to employment. Luithlen (259) contributes a brief survey of the present status of apprentice selection in a large German industry. Validation data for the tests referred to by this author are included in an earlier article by Heilandt (176).

A special study of tool making apprentices forms part of a general investigation of intelligence tests for employment, reported by Pond (317). Eight tests and a personal history questionnaire are given to three groups of apprentices. Each test and a combination of the most significant of them are compared with foremen's ratings, and a preferred range of scores established. The percentage of satisfactory workers in the preferred range of total scores is 82 and in the non-preferred range, 23, while among all apprentices tested, it is found to be 60. Use of these tests reduced the number of unsuitable apprentices among those hired, from 18 per cent to 0.

The general investigation by Pond (317) includes an evaluation of the same eight tests and of personal history items in the selection of workers for 65 occupational groups in a brass factory. The methodology is such as to give preferred ranges on individual tests and items, and on combinations of tests, which permit every applicant to qualify for one or more positions in the plant. This study is of unusual scope from the points of view of objective, of number of workers examined, of the periods covered, etc., and involves a painstaking analysis of criteria of reliability, and of the reliability and validity of individual and combination tests. It is characterized throughout by a highly conservative and judicious attitude in the interpretation of data and in the formulation of conclusions with respect to the usefulness of the tests, etc.

The validation of twelve analytic tests for use in the selection of workers and apprentices for foundry work is described by Bültmann (79).

A variety of factory occupations is included in a study by Walther

and Ehinger (459), also described in separate articles by Walther (460, 461), who find a correlation as high as $+1.00$ between ratings of 230 workers engaged in 11 departments of a food product plant, on five tests of motor ability. An uncritical use of correlation methods and a disregard for certain contradictions which appear in the results temper the scientific value of this study. Heilandt (177) describes a battery of analytic tests and administrative procedures employed in the selection of workers for a number of specialized factory operations involving intensive training. The results of a preliminary follow-up are included in the article.

Various methods employed in the selection of metal trade workers are compared by Toltschinsky (433), who criticises the assumption that it is possible to use a single series of tests for an industry including over 100 specialized activities, each of which is characterized by highly diverse operations.

Among articles on other factory occupations is an analysis of acetylene welding and an investigation of selection tests for this work by Radler (326). An analysis of the qualifications of crane operators and a preliminary evaluation of tests of competency for this work are included in an article by Oberhoff (297). Tests for glass blowers are described by Bolt (66).

The aptitude of printers has been the subject of other investigations. Taylor (424) finds differences in average intelligence test scores of pre-apprentices, compositor apprentices, and press apprentices. In a more extensive investigation by the same author (425) seven tests are applied to approximately 800 apprentices, including 40 hand compositors, 70 linotype operators, 50 stone hands, 110 job and cylinder pressmen, 145 newspaper pressmen, and to 800 pre-apprentices. The test scores are compared with the rather uncertain criteria of ratings by instructors in the apprentice schools, with performance on job samples, etc. The findings are viewed by the author as helpful in envisaging the problem of printing aptitude, which requires much more complete analysis. Hall (172) reports a correlation of $+0.58$ between the Minnesota Paper Form Board Test and ratings of from "Poor" to "Exceptional" by instructors of 89 apprentices attending a school for printing pressmen. Although there is the usual difficulty with the criterion, the results are of interest in confirming the reliability of the test and in connection with the general findings included in the article by Taylor (425). Christaens (89) describes a re-evaluation of tests for printers, revealing differences between "Good" and "Poor" workers.

In a study of 341 apprentices in the hand sewing trade, Stolley (413) administers seventeen tests and compares test scores with an objective work task of preparing clothes for a doll and with general trade achievement.

Michalewa and Kujawesa (275) describe the evaluation of twenty-four tests for machine and hand workers in a tailoring plant. Satisfactory correlations are reported in the study of 57 apprentices divided into three groups on the basis of ratings by the supervisor of apprentice training. Eighteen tests are used by Treat (437) in an attempt to predict the ability of subnormal girls to operate power garment machines. Five of these show a correlation of $+0.66$ with the criterion of "probable success," this correlation being higher than between chronological age, mental age, or I.Q., and the criterion.

In the textile industries, Schneider (354) employs Poppelreuter's method of analyzing work curves in predicting the success of workers in a silk mill. The study is of particular interest from the viewpoint of methodology, involving an analysis of the job by time study techniques. Lewin and Rupp (245) describe the evaluation of tests for operators of spinning machines and for dressmakers.

Tests of bookbinders are described by DeCroly (117).

Electric substation operation is investigated by Viteles (450), who includes in his article data on the validation of selection tests by comparison with the working efficiency of substation operators, senior operators, and assistant operators. Both an objective criterion of operating errors and a subjective criterion of ratings are employed.

The Bureau of Public Personnel Administration makes its contribution to the quota of unstandardized and unvalidated tests in the form of suggested tests, generally combining trade and aptitude tests in paper form, for farmer (398), baker (399), steam fireman (23), instrument men (24), head laundry man (387), rod man (25), laborer (277), blacksmith (388), water meter reader (389).

B. Tests for Office Occupations

Among contributions on tests for office occupations is a survey by Freyd (141) of tests of aptitude, proficiency, and education, which have been used in the selection of typists and stenographers. Data on the evaluation as well as descriptions of the tests are included. Somewhat the same information for fourteen tests of clerical ability is included in an article by Anderson (7) which closes with pertinent critical remarks on these tests.

The predictive value of general intelligence tests in the selection of junior accountants and bookkeepers is discussed by Scudder (367). The subjects of the investigations are rehabilitated war veterans. General relationships between general intelligence and degree of rehabilitation in these occupations are revealed. Data obtained in a comparison of scores on standard tests of proficiency and of aptitude with progress in typewriting, reported by Stedman (407), are of interest from the viewpoint of the use of these tests in selecting typists.

Trade and ability tests are included in batteries of tests for use in selecting workers for clerical occupations in an investigation by Munro and Raphael (285). The number of cases is small in every instance, and only in the case of Hollerith operators is an objective criterion employed. Reference is made to tests for six other clerical occupations. Low correlations between grades on an achievement examination and psychological test scores on one hand, and supervisors' ratings of 200 office employees on the other hand, are reported by Braunshausen (73). The author explains the low correlations in terms of the failure of tests to measure character traits. Weigl (463) contributes an article on the validation of six tests in comparison with ratings of fifty-two office employees of a Dutch commercial firm. The tests are of the analogous type, specific to work situations.

Tests for Hollerith machine operators are described by Couvé (93).

In an article by Dixon (108), procedures employed in the selection of cashiers are reported. Psychological test scores and personal data obtained in a clinical interview are combined to give "satisfactory" results. However, the author's assurance that the procedure gives "good results" represents the only reference to validity.

A further contribution to the already long list of tests without known validity in the selection of office workers is made in the form of suggested tests for stenographer (390), secretary-stenographer (26), statistician (391), senior statistical clerk (392), senior file clerk (393), senior clerk (394), addressograph operator (27).

C. Tests in the Selection of Transportation Employees

Little progress in the development of new tests for transportation workers has been made in recent years, attention being centered on the re-evaluation and critical comparisons of existing techniques. A section of a comprehensive volume by Herwig (181) is devoted to tests for operators of all types of motor vehicles. The description of Münsterberg's work is followed by descriptions and highly critical

evaluations of techniques employed by Faust, Moede, Piorkowski, Tramm, those used on German railways, etc. A section of this book is given over to a consideration of the tests used in the selection of other transportation workers such as yardsmen, transportation supervisors, etc. A similar review of investigations in the selection of motor vehicle operators is embodied in an article by Hallbauer (173), representing a source of much valuable information to those interested in this field. Among tests classified by the author as analytic in character he discusses and critically evaluates the tests of Moede, Piorkowski, Tramm, Viteles, Lahy, etc. Among synthetic or analogous techniques, involving an attempt to measure the global situation, he includes the tests of Rupp and Stern. Tests of proficiency of speed are treated separately. The reader may disagree with Hallbauer's classification and with his critical comments, but these are mere details in this relatively short but thorough review of experimental studies in the selection of operators for motor vehicles.

A full description of tests of speed of reaction, distribution of attention, mechanical ability, judgment of speed and distance, etc., used by Lahy in the selection of motormen are described in a volume by this investigator (233). An analysis of the job and of the qualities presumably measured by each test, detailed data on procedures, on distribution of results, on the scoring of each test, etc., are included. Validation data are reported for 200 men with a criterion which has not received the same careful study as it has been given, for example, by Shellow (371). The practical usefulness of the test appears in a statement by the director of the transportation company that only 3 per cent of employes are found unsuitable to employment, in contrast to the 20 per cent before the tests were used, and that great economies in training cost and accident cost have resulted from the use of these tests.

A new test for the selection of motor vehicle operators is described by Forster (136), who insists upon the importance of time of choice reaction to various sensory patterns in safe driving. The author notes a relationship between success in a choice reaction test and general intelligence. He also suggests that tests of intelligence, reaction time, and span of sensory perception can replace every other kind of special examination for drivers. No validating data is presented to bear out this conclusion.

Johnson (200) quotes Snow and Wechsler in recommending a mental age of twelve years as a qualification of safe driving. Snow (380) finds greater turnover at the end of a month among men

with low scores on a general intelligence test than among others. A reference to tests for cab drivers is found in another article (381) by the same author.

Of significance from the viewpoint of the selection of motor vehicle operators is an analysis by Kunze (228) of factors which intervene in perception of speed of movement. He tests the visual factor as reflected in estimating the speed of movement of images projected upon a screen; auditory factors by discrimination of differences in tones of a moving motor; control of equilibrium, etc. He then submits each of twenty-six subjects to a complex global test of a perception of speed of movement such as occurs in a moving automobile. A correlation of $+0.89$ between the auditory test and the global test leads him to the somewhat premature conclusion that the auditory factor is the most important in estimating the speed of a moving vehicle. Dembitz (103) also reports an investigation on the visual perception of movement.

A small electrically driven motor and a test requiring the subject to follow a printed path on a moving belt are used by Poppelreuter (320) to study tendencies in the operation of a motor vehicle. Among the types of operators isolated in this study are the "reckless," the "anxious careful" type, the "differentiated careful," etc. Apparatus used in this investigation, embodying the work sample viewpoint of the author, is recommended for use in the selection of operators.

Shellow and McCarter (371) describe an improved method of rating for use in rating the operations of a motorman to arrive at an index of his proficiency.

D. Tests for the Professions, Engineers, Executives, Etc.

The total absence, during the period covered by this review, of technical publications in English on the selection of salesmen, suggests a lack of interest in a phase of selection which received considerable attention a few years ago. The problem of selecting salesmen is attacked in a few German investigations. Müller (284) finds a correlation of $+0.65$ between five tests and ratings of sales people. The number of subjects and other pertinent data are omitted from the report. Moede (280) describes another battery of tests employed in the selection of retail sales apprentices and presents an equally unsatisfactory report on validation. Bogen (65) passes in review the problems entailed in the development of scientific techniques for the selection of sales personnel. Difficulties in the analysis of quali-

fications, of providing suitable criteria, the importance of character traits, etc., are discussed. Stern (411) also contributes a brief exposition of principles and refers to investigations in selection of employes for mercantile establishments.

A specialized phase of selecting is made the subject of investigation by Wagner (456), who employs technical problems and measures of intellectual capacity in the selection of sales engineers in the metal industries.

Strong (418) reports further data on the interests of engineers, and suggests the possible use of the Interest Inventory, with revised scoring, for the selection of engineers for the electrical industry. The interest of engineers as compared with other occupations is treated at length in another article (417). The question of the validity of these tests for predicting engineering ability remains unanswered.

Remmers (328) develops another interest inventory to distinguish between students interested in engineering and those interested in agriculture. The inventory is open, as the author recognizes, to the same questions concerning purpose, reliability, validity, serviceability in selection, and guidance, as are other tests of this type. A brief review of interest measurement and a bibliography are included by the author.

Strong (419) applies his Interest Inventory to eighty executives and analyzes similarities and differences of interest in comparison to those of men in allied professions. His results lead him to question the possibility of using the inventory to isolate executives as a distinct group. The author discusses the hypothesis that there exists a class of executives as distinct from other occupational groups. The Allport A-S, C-2 Personal Inventory, and a Directions tests are used by Beckman and Levine (53) in an attempt to find a test of executive ability. Only the first shows any sign of validity in a specific situation involving city executives, ratings on efficiency, etc. Bartlett (43) describes a test of business knowledge used in selecting men for non-sales supervisory positions. A somewhat similar test is described by Hepner (179).

Strong (420) uses the Interest Inventory to distinguish between ninety-nine certified public accountants and other occupational groups.

According to Davenport (100) inspectors score higher on introversion, as measured by the Personal Inventory, than do foremen.

Of interest from the viewpoint of predicting success in the professions is the work of Roe and Brown (330) on the analysis of qualifications for dentistry, and the very important contribution on the measurement of scientific aptitude by Zyve (480).

In the field of the professions there are also suggested partially standardized tests for the occupation of junior personnel examiner (401), laboratory assistant (402), medical social worker (403), occupational therapist (404), dietician (405), store-keeper (400). The fact that no material on validation is available makes the scientific value of these tests nil.

E. *Miscellaneous Tests*

In addition to the experimental studies described above, there are a few investigations on selection tests which do not fall readily into any of the groups listed above. There are also a number of studies not primarily concerned with the selection of workers which should, nevertheless, be brought to the attention of those interested in the application of psychology in industry.

Among the former is a number of articles dealing with the selection of policemen. O'Rourke (300) summarizes standards and procedures employed in the selection of policemen in sixty American cities, and describes a battery of tests developed by the Research Division of the U. S. Civil Service Commission for the selection of policemen in the District of Columbia. Psychological tests employed in the selection of policemen in Europe are reviewed by Viteles (451), who includes examples from the work of Schulte (359) in this field. The author draws attention to the inadequacy of the criteria employed in most of these studies. An article on tests used in the selection of policemen in Bavaria is contributed by Graf (166), who gives much material on qualifications, on administrative procedures, but little experimental data. The intelligence of policemen in Palo Alto and its relation to survival on the police force are discussed by Merrill (272). In the literature of the period are found suggested and partially standardized tests for police sergeants (28), policewomen (29), and a report of selection of policemen in Syracuse (30).

Material on the selection of Army recruits is embodied in a volume on *Psychology and the Soldier*, by Bartlett (45). Awaji (42) reports satisfactory results obtained from the application of group tests (modelled on the pattern of the American Army Tests) to 6,000 recruits of the Japanese army. Positive relations between the O'Rourke classification test scores and fitness for service of 1,000 Navy recruits over a period of one year as measured by promotion and incidence of delinquency; and negative relations between test scores and delinquency are reported in an interesting article by Cummings (98). An article by Klutke (213) embodies a description

of tests used in the German Post Office service for the selection of telephone operators, telegraphists, etc.

A group of "psycho-motor," "sensory," and "mental" tests are used by Drill (114) in the selection of twenty-four firemen. Modified trade tests, involving the ascent of a ladder and the search for objects in a room filled with smoke are reported as showing the most satisfactory correlation with ratings by fire chiefs, after nine months of service. Positive relations between scores on a group test of mental ability and the degree of efficiency of delivery men, as determined by ratings, bonus reductions, and turnover, are noted by Johnson (201). Knoop (222) describes a battery of tests used in selecting 30 workers from among 125 applicants for service with a company engaged in furnishing a service for ordering cabs.

Among articles having a bearing on the selection of workers, although not concerned with selection for specific occupations, is a description by Feist (133) of tests which have been used by Poppelreuter, Giese, and others, in measuring "presence of mind" in fear producing situations. The suitability of Piorkowski's "attention and fatigue" meter as a test of attention is experimentally investigated by Verwoerd (447), whose careful analysis shows no justification for the suppositions underlying the use of the test in the measurement of distribution of attention. A test useful in examining workers for occupations involving the regulation of temperature is described by Liebman (247).

Tests suitable for the measurement of spatial perception and spatial imagery and problems involved in such measurement are discussed by Krüger (225), and by Braunschweig (72). Dietz (105) comments critically on procedures for the administration and scoring of tests of the ability to estimate the size of lines, angles, etc., and describes a test for the estimation of length of lines.

The measurement of technical intelligence is the subject of discussion by Lossagk (255). Schultz (360) describes a mental alertness test held to be suitable for the selection of junior employes. Further data on the reliability and validity of tests of musical aptitude is found in articles by Brown (77), Larson (241), Stanton (406), Gaw (153), and Nielsen (294). Meier (269) contributes a report in an essay on the measurement of art talent.

The existence of individual differences among workers on farms and the use of tests for measuring such differences are discussed by Dunlap (115).

Among other miscellaneous tests are partially standardized tests for housekeeper (396), and for food inspector (395).

F. Trade Tests and Rating Scales

The trade test as a measure of vocational fitness continues to receive scant attention. A few of the tests referred to above, particularly those suggested by the Bureau of Public Personnel Administration, involve trade test situations, but there are no titles dealing specifically with such tests.

There are likewise few titles on rating scales in the period covered by this review. Kornhauser (216) completes his series of excellent articles devoted to a statistical analysis of rating scale techniques. The final article, given over to a comparison of ratings on different traits, includes data on the relative standing of traits with respect to the reliability of the ratings on these traits, findings on the intercorrelation of traits, etc. That ratings are not wholly worthless on purely statistical grounds; that, judged by non-statistical criteria they may be of real value, and that procedures for rating can be improved, are the general conclusions following from Kornhauser's careful study. In contrast to the thoroughness of Kornhauser's evaluation of the contribution of rating scale technique is Bartlett's (44) uncritical eulogy of the rating scale as a selection instrument. Kneeland (214) demonstrates the existence of a lenient tendency in rating by reference to the use of three scales involving close to 2,000 ratings in connection with the study of clerks and section set-up in a department store. The improvement of a scale for rating motormen by the substitution of specific operations for descriptive adjectives is reported by Shellow and McCarter (371).

G. Non-Test Factors in Selection

A summary of earlier work on non-test factors in selection is included in the volume by Hollingworth (189) to which reference has been made. Of interest to industrial psychologists is Kornhauser's (218) determination of the value of opinions given about students entering college by former teachers, employers, and others in a comparison of four reference reports. There is a definite need for the application of similar procedures in evaluating the use of reference report forms in the selection of workers.

A unique and fruitful approach to the investigation of possible prediction of vocations and success from photographs is employed by Landis and Phelps (238) in an investigation of the judgment of forty photographs of successful and unsuccessful men in four vocations. The results, reported in detail, show that a group of untrained judges are unable to determine vocation and relative success

from the examination of photographs. A repetition of this experiment upon trained observers in industry would be desirable. Of general interest from the point of view of judging intellect from photographs is an article by Gaskill, Fenton and Porter (152).

Character analysis is further "debunked" in an article by Laird (236). On the other hand, Engelmann (126) finds that a "first impression" of the ability of printing apprentices, obtained in thirty seconds, agrees with psychological test scores in all but two of twenty cases. However, the twenty cases appear to be a somewhat selected group from 273 apprentices. Meili (270) shows that character judgments made by "chance" coincide with the subject's estimates and satisfy him in as great a number of cases as those made on the basis of an examination of the electrical resistance of cranial areas—in connection with a system of character analysis which has recently obtained great favor in Germany and Switzerland.

The importance of the interview in the selection process is stressed by Anderson (9) in a clear, well organized, and detailed statement on the kinds of observations to be made by the interviewer, on the necessary technical training and the experience of the interviewer, etc. The article embodies an excellent defense of the clinical interview and selection, and an evaluation of the relative contribution of psychological tests and of other techniques in the estimation of total personality to be made in the interview. However, Anderson fails to suggest the importance of applying to interview techniques the same objective standards of evaluation upon which the psychologist insists in the case of other procedures which he employs in the diagnosis of personality. An attempt both to apply interviewing techniques and to evaluate them in connection with measurement of character traits in the selection of prohibition officers is described by O'Rourke (301). That there are elements of error in the procedure is recognized both by the author and in an editorial criticism of this work (31), but it represents, at least, a step in the direction of standardizing and objectifying the interview. An annotated bibliography on the interview is provided by Moore (282).

The possibility of using handwriting as an index of vocational and other aptitudes continues to receive attention, particularly at the hands of European investigators. In German there appears to be a frame of mind receptive to graphology. An indication of this is to be found in a new edition of the classic work by Klages (211). Another volume by the same author (212) includes a description of the evaluation of handwriting of individuals in many occupations by

twelve graphologists in four countries. A volume by Saudek (349, 350) represents another major contribution in this field. Hall (171) concludes that handwriting furnishes a suitable criterion for selection in commercial occupations from a study involving rating by thirty-four judges of the competency of nineteen students in a commercial school, and a correlation of ratings by a graphologist with ratings on accomplishments, etc. Kügelgen (227) finds that ratings on the moral qualities of forty-eight printing apprentices, based on an examination of handwriting, coincides with tests of mental ability and intelligence in 66 per cent of the cases, and nearly coincides in 21 per cent of the cases. Bobertag (64) reports on the comparison of character judgments of five subjects by 18 acquaintances and by six graphologists. The graphological ratings, according to the author, appear to be fairly valid. However, the graphologist will find little consolation in a comparison by Schorn (356) of the judgment of five graphologists on one subject. A keen analysis by the author shows marked disagreement among graphologists, and a ubiquity of "empty phrases" and of statements of very general application. The need for more carefully controlled evaluations of the contribution of graphology in the measurement of aptitude and of character is pointed out by the author.

The effect of social status, age, education and related factors in occupational success is the subject of a number of investigations. An excellent example of this type of investigation, including a study of the relationship between success in college courses, extra-curricular activities, organizations in college, to success as an employe of the Bell Telephone Co. is reported by Bridgman (375). Ching (88) shows the influence of age, training, academic and other experiences on progress in a group of scientific occupations. The significance of age, education, intelligence, etc., in relation to success as a sections manager (186) in a department store, as a sales clerk (187), and as a messenger (188), is described by Ho. That social status of parents does not influence the length of service in clerical occupations is the conclusion of a study by Bergen (55). The effect of residence in city and country upon success as an apprentice machinist is discussed by Krüger (224).

H. Analysis of Occupational Qualifications

The study of occupation qualifications continues to receive little attention in the literature as a distinct item. Both the techniques of job analysis and findings from the application of its technique are discussed in many of the articles on tests described above.

Geissler (155) discusses the need for job analysis and describes the form used for this purpose in the textile industry (154). The technique used in analyzing jobs to determine their suitability for handicapped girls is described by Dye and Unger (118). Feick (132) analyzes the activities characteristic of workers in various bank jobs and presents a highly subjective analysis of the necessary mental qualities associated with these activities. Ruffer (338) classifies 100 specialized office jobs into 19 groups and assigns a rating on the degree of competency for each group on such qualities as logical memory, persistence, etc. Breithaupt (74) points to the possibility of using the records of penalties imposed in the industrial plant in analyzing desirable qualifications. Tests of finding rhymes, completion of poetic fragments, association of verbs with nouns, etc., are employed by Stumberg (422) in an analysis of poetic talent.

Of general interest, from the viewpoint of job analysis, are the sample job specifications published by the American Council on Education (5) and statements on qualifications and psychological and sociological factors affecting success in occupations for women by Hatcher (175).

III. APPLICATION OF PSYCHOLOGY IN MERCHANDISING

In an earlier review (454) it has been pointed out that the application of psychology in merchandising has taken two directions, (1) an attempt to rationalize principles of human behavior, as revealed by laboratory investigations, in terms of the specialized performances described as buying, selling, advertising, etc., and (2) an attempt to set up experimental conditions, that is, to apply scientific techniques in investigating the factors influencing these forms of human response.

Among major works appearing in recent years illustrating the first tendency is a volume by Eichler (124) embodying a series of generalizations, not experimentally verified, on the psychology of selling. This represents an example of the tendency referred to in earlier reviews toward a "practical common sense" but not always valid application of psychology to selling.

In somewhat the same category is the discussion of argument and suggestion (patterned somewhat after Scott (366)) in outdoor advertising (303), and a book on practical salesmanship by Pelz (309). Swift (423) includes didactic generalizations on selling in chapters of a volume devoted to methods for influencing human conduct.

A characteristic example of the second tendency is a monograph by Roloff (332, 333) given over to a description of an experimental study of the effectiveness of posters. Following the failure of a jury of specialists to agree in the selection of eight posters from among 77 submitted in a contest the synthetic and aesthetic value of the posters and their forcefulness were studied by obtaining ratings from about 100 subjects, each of whom observed the posters for a period of 15 minutes. Ratings by these observers, as well as by 300 readers of an advertising periodical, confirm the judgment of the members of the Committee who were engaged professionally in the practice of advertising. The purpose of this study, the methods employed, in spite of possible weakness in detail, make it a significant experimental contribution in the field of merchandising. It is interesting, in passing, to note in an article by Karsten (204) another example of an experimental evaluation of designs submitted in a prize contest.

A volume by Franken and Larrabee (138) also furnishes an example of the application of experimental methods to problems of advertising. The authors find, in the course of a series of experiments conducted on students of New York University, that packages colored orange and yellow give the impression of greatest size. Preference on the part of this highly selected group for other factors in connection with packages is also tested. In the same group is an earlier publication by Hotchkiss and Franken (193), reporting the use of the controlled association method in investigating the recall of commodity names and of manufacturers' names and brands. Variations in familiarity over a period of years, the effect of advertising upon recall, sex differences, advertising policies, etc., are analyzed in this detailed experimental study.

In the case of most of the larger contributions on the psychology of merchandising, the two tendencies referred to above are combined in varying degrees. So, for example, Greenly (169) combines a reference to experimental work with a general discussion of psychology as a sales factor. In an interesting, well written book by Marbe (264) stress is laid upon the dependence of practical advertising upon psychological theories and principles. Brief descriptions of investigations in this field are included, although the emphasis is on the bearing of principles developed by the experimentalist in the psychological laboratory. Hahn (431) contributes a translation of a volume on the principles of advertising by Tipper, Hollingsworth, Hotchkiss, and Parsons.

The journals furnish a number of shorter general and experimental articles on the application of psychology in merchandising. Experimental studies on selling are fewer than those on advertising. Among the few experimental studies in the former field is an essay in the investigation of personality as a factor in sales success. The author (146) uses a test of introversion and extroversion involving an observation of differences in the intensity of tones heard during reverie and while the subject is attending directly to the stimulus. Results of 30 subjects, compared with ratings, lead the investigator to the optimistic conclusion that in extraversion of personality, as measured by these tests, is to be found the secret of sales success. Bergen (56), on the other hand, finds that the real secret of success in selling is in the use of suggestion. Donaldson (112) outlines 5 criteria of personality expression in business. A shopping tour by 120 students is used by Phillipps (312) in a study of glaring defects in retail selling.

In the case of advertising, the experimental approach is embodied in a number of studies characterized by a careful control of the variables under observation. In some instances, the studies represent the further investigation of problems upon which the authors have been working for some years.

An example of the application of laboratory techniques and standards in the examination of advertising problems is to be found in an investigation by Nixon (295) of differences in the time taken to differentiate advertising from nonadvertising matter in newspaper and periodical publicity. The results obtained by the use of a special tachistoscope and 49 subjects, constituting a fairly representative sample of the population (a condition overlooked in many experiments in this field), and colored and noncolored editorial and advertising matter indicate that color makes little difference in the speed of perception of advertisements, and suggest the desirability of the use of "hard to perceive" styles of layout by continuous advertisers.

Burt and Crockett (82) apply to advertising posters the laboratory techniques employed in studying recall and recognition of publication advertising in driving 19 observers by two series of 40 posters at the rate of 20 miles an hour. The authors cite results on the influence of primacy and recall, the relative recall of picture ads, commodities, trade-marks, etc., but point to the many uncontrolled variables affecting the results in this experimental situation.

The relative significance of intensity, repetition, and allied factors

influencing recall of advertisements continues to receive the attention of investigators. In one experiment by Adams (3) 100 students, familiar "dummies" and techniques are employed with changed material—single words on quarter, half and full pages—to show that the isolation of a single word on a full page has no effect on its recognition value, but that repetition is effective. That there are applications of findings in this artificial situation of significance for advertising is, of course, questionable. In a later experiment by Adams and Dandison (4), who employ colored squares of various sizes, the relative recall value of one unit of area is shown to be 1.00, of two units of area 1.6, and of four units of area 1.68. Another experiment, involving the use of four-letter words in various arrangements, reveals a distinct rise in curve of recall after five repetitions of the word. With a variation of procedure significant differences are revealed, indicating increases in attention value of size and in memory value of repetition.

The value of absolute size is examined by Newhall and Heim (293), who show one of three sets of ads of various sizes to one of three groups of college students. Results obtained through recall by the method of paired associates lead to the conclusion that the value of commercial advertising is apparently independent of absolute magnitude.

The belief in the superior "drawing power" of the positive advertisement is well intrenched among other traditional beliefs. The validity of this belief is investigated by Lucas and Benson (256) in a comparison of returned coupons for ads matched for time of appearance, periodical, size, etc., but differing in direction of appeal (*positive* versus *negative*). The percentage of coupons returned and cost per returned coupon are studied. The findings show no difference in appeal between positive and negative ads, no sex differences, and that variations within each of the two types are greater than the differences between the two. The findings are highly suggestive, although they give no clue as to whether the doctrine that unpleasant experiences are suppressed in memory applies to advertising. The historical method favored by Kitson is applied by the same authors (258) in studying the trend of advertising since 1912, which shows an increase in advertisements with negative appeal since 1922—an increase of 12.3 per cent in 1922 to 35.2 per cent in 1927. Disagreement with Kitson's earlier findings are discussed and explained by the author.

Strong and Laslett (421) offer further evidence in support of

the superiority of the "want" as compared with the "commodity" advertisement obtained in an experiment involving 108 men and 18 women in a general psychology course.

Asher (39) uses results from a comparison of a paired associates test with expenditures in newspaper advertising to study the influence of such advertising upon familiarity with retail stores. Although recognizing the complexity of the variables involved, the author interprets his data as indicating that advertising is the most important single factor in determining familiarity with retail stores.

In a study by Schmidt (353) is carried further the analysis of confusion of designs (trade-marks) undertaken earlier by Marbe. The experiment, characterized by a careful control of such factors as intervals between presentation of designs, degree of resemblance, etc., lead to conclusions such as (1) frequency of confusion varies directly with interval between the presentation of two series, (2) accuracy of recall varies directly with length of series, etc.

Bartlett (46) indulges in highly metaphysical speculations on mental factors underlying the control of the "will to buy" through advertising.

During the period covered by this review additional evidence on the legibility of various type bases has been obtained. Tinker and Patterson (428) find that 640 subjects read material in lower case types 13.4 per cent faster than that in all capitals (as against 10 per cent in Starch's experiment). The rate of reading material in capitals is 2.8 per cent faster than that of reading italics. Different forms of the Chapman-Cooke Speed of Reading test are used for reading material. In a later article by the same authors (429) on the legibility of the size of type are outlined the requirements for an experiment in this field. Results are reported showing that 10 point type yields the fastest reading of the five types used under the conditions set up in this experiment. In a still later article on the length of line (430) the recommendations of other investigators are reviewed. An experiment, involving the use of 10 point type, shows the superiority, from the viewpoint of rate of reading, of the 80 mm. line. The optimum length of the size of line is said to be between 59 and 97 mm., but closer to 97 mm. The relative effects of line changes on slow, fast, and medium readings, are discussed in the final article of this model series in this field of investigation.

From the results of a somewhat more involved experiment designed to determine the relative legibility of Intertype Ideal New

Face Type, set up in standard newspaper form, as compared with other types, Hovde (191) concludes that context is more important than form of type in determining legibility when reading rate is used as a criterion. Preference of type appears to give no clue to legibility.

Burt and Beck (83) report a well arranged experiment to determine the legibility of three types of "backbone titles". The results show the consistent inferiority of one type and the desirability of considering for use another seldom employed.

Advertising as used by the German railways is discussed by Sarter (348), in the electric lamp industry by Prox (323); Kurtzig (229) expresses the need for exact data on the value of film publicity.

IV. OTHER INDUSTRIAL APPLICATIONS

The writer has discussed in an earlier review the interest of the industrial psychologist of Europe in problems investigated in this country by industrial engineers. This difference in attitude between European and American psychologists is also referred to by Kornhauser (217) and is further reflected in the contents of many of the foreign books and articles reviewed above under "*Scope of Industrial Psychology*". The inclusion of chapters on methods of work and the effects of environment in Burt's (84) recent book may suggest a growth of interest on the part of American investigators in what Giese has termed the "object psychology" of work, but, actually, American technical journals remain about 99.44 per cent pure in so far as interest in the materialistic aspects of industrial surroundings are concerned. On the other hand, in European journals numerous articles appear on the effect of ventilation, of illumination, and of other conditions of work, as well as articles on time and motion study, machine construction, etc.

As in the earlier reviews (454, 455) and for reasons stated there, no attempt will be made to cover in the present summary studies of the types mentioned above. The reader interested in such studies is referred to the *Journal of the National Institute of Industrial Psychology*, *Industrial Psychotechnik*; *Psychotechnische Zeitschrift*; *Revue de la science du Travail*, the reports of investigations by the *Industrial Health Research Board of Great Britain* (formerly *Industrial Fatigue Research Board*); and to excellent summaries of these investigations appearing from time to time in the *Journal of Personnel Research* and in *Psychological Abstracts*. There are,

however, a number of contributions on the application of psychological principles and techniques in analyzing the causes of accidents, in the analysis of motives, in studying the attitudes of workers, and on similar problems of general interest from the viewpoint of "subject psychology" of work (to adopt Giese's terminology), which have been selected for review.

Among major publications on the applications of psychology in industry, exclusive of vocational selection and merchandising, is a book by Burt (84) with chapters on training, fatigue, methods of work, satisfaction and morale, etc. This well organized and well written volume, embodying a wealth of data from experimental studies and an appreciation of the contribution of laboratory psychology, represents a valuable addition to the literature of industrial psychology.

(a) *Accidents*

The industrial *accident* as a specific problem is considered in a number of recent American and foreign publications. Farmer (131) summarizes findings reported in earlier studies on the influence of individual differences and of other factors in accidents. In a series of articles by Bingham (63) and by Slocombe (373, 374, 375) are reported the findings of an investigation on the causes of accidents on the part of transportation workers and reduction of accidents through individualized study and treatment of accident-prone employees. Of particular interest, in addition to the confirmation of the existence of individual proneness to accidents and the analysis of factors underlying proneness, is the application of the clinical method to counterbalance the factors creating susceptibility towards accidents on the part of the individual.

Accidents involving motor vehicles is also the subject of an article by Ach (1), who insists upon an inherent conflict between speed of operation of motor vehicles and the psychophysical capacity of the individual to react quickly and accurately enough at these high rates of speed. The control and registration of speed, education of pedestrians, etc., are recommended as ways of avoiding transportation accidents. The significance of reaction time in the application of the brake, as a factor in vehicular accidents, is also stressed by Tramm (435). Psychological problems in railway accidents are discussed at length by Grüb (170).

The influence of individual differences in accident causation is pointed out by Viteles (453). A study by Stevens (412) of 5,000

cases receiving compensation for injuries at work shows a high positive correlation between duration of disability and age in the case of nearly all injuries.

Significant sex differences in susceptibility to automobile accidents are reported by Viteles and Gardiner (452), who find that women cab drivers have three times as many accidents per thousand miles of operation as men driving similar equipment under similar conditions.

Susceptibility to accident is also studied by Hildebrandt (182) in a comparison of three apprentices with accident records and three apprentices with records of no accidents. An analysis of findings on psychological tests and ergograph curves lead the author to conclude that the first three apprentices experience accidents because of instability and by reason of incapacity to concentrate attention unless interest is strongly challenged in the work. A critical discussion of the significance of differences in reaction time is included in this article.

Illustrative of investigations on the influence of conditions of work upon accident rate are to be found in recent publications of the Industrial Health Research Board of Great Britain. So, for example, significant differences in accident traits between poorly and well ventilated mines is reported in a number of studies (440, 442). Variation in incidence of accidents with output and with turnover, with distance of seam from mouth of mine, its age, are also reported in the case of miners in these studies (440, 442). Conclusions concerning the influence of illumination on accident rate are cited by Polakov (315). The contributions of scientific management to accident reduction are discussed by Martens (265).

Figures on frequency of accidents under day and piece work systems of payment, in which the latter is associated with a lower accident rate, are published by Lippman (253). However, the author cautions against drawing final conclusions from these figures because of the presence of variables other than systems of wage payment.

Brakeman and Slocombe (69) contribute a painstaking review of experimental results relative to the study of individual accidents and susceptibility under the headings of vision, reaction time, fatigue, attention, kinæsthesia. The review is limited to laboratory investigations which have a bearing on the practical industrial situation. As a result of this restriction and of the absence of German titles many findings and conclusions of utmost importance from the view-

point of accident susceptibility are missing from this otherwise excellent summary.

B. *Training*

In addition to chapters on *training* in a number of the texts mentioned elsewhere in this review there have appeared a few books and a number of articles specifically devoted to training and involving an application, to a greater or less degree, of psychological principles in training workers. The systematic instruction of motormen, of workers in shoe factories and in the metal trades is used by Carrad (364) in illustration of the theory and practice of training on psychological grounds. The factors to be considered in the organization of an industrial training program are discussed by Moede (281). Rupp (343, 344) discusses the foundations of a "vocational pedagogy". He weighs the relative advantages of direct training in the factory and in trade schools, and the significance of such factors as interest, incentive, etc. Of particular interest to the industrial psychologist is his criticism of the practice of isolated units of a trade activity such as holding the hammer, grading the force of the blow, etc., as recommended by Berling (57). The latter employs training in the use of the file divided for practice into such elements as rhythmic movement, maintenance of constant pressure, etc., in illustration of the "part" method of training, recommended by him. A contribution to this problem of training is made by Dilger (106), who compares the progress of two groups of workers, as trained in the use of the file, (1) through practice on the whole activity on production work, and (2) by means of special tools and special procedures. His results point to the superiority of the former method.

Nebel (290) illustrates the use of careful time study, of moving pictures, etc., in formulating the best methods of filing for purposes of training. The author refers to reductions in training time following the introduction of such training methods. He also briefly reviews the earlier literature on training.

Findings and conclusions of significance from the viewpoint of training manual workers are, of course, to be found in a number of the articles discussed above, under the caption of "The Measurement of Motor Capacity".

In view of the few objective studies on the value of organized training, there is unusual interest in a comparison by Krüger (226) of systematically trained machinist apprentices with apprentices not

profiting from systematic training. The benefits of organized training are demonstrated in this study, involving the preparation of stock articles by apprentices. The author describes the methods employed in training, which include regular practice on isolated operations.

The intimate relation between scientific selection and training is referred to by Koblanck (221) and by Bolt (67), who use in training exercises similar to those employed in selection.

Developments in training for specialized occupations, in part through the application of psychological techniques, are described in a number of publications. Outstanding among these is a detailed description, by Schwarze (363), of methods and materials employed in training workers on the German railways. Couvé (94) also writes on this subject. Among other contributions of this sort is material on training transportation workers by Herwig (181); on locomotive engineers, by Schuschakow (367); on sales people, by Tumena (438); on glass blowers, by Bolt (67); on clear enunciation for long distance telephone operators, by Schuck (357).

The significance of habit interference and related forms of behavior in training transportation workers is discussed by Viteles (453). Kneeland (215) outlines a set of arbitrary rules for the use of the interview in training sales people. Dixon (109) refers to the development of a training class for increasing the "personal efficiency" of supervisors.

Of general interest, although not primarily oriented from the viewpoint of the psychologist, is a verbose but detailed description of procedure for organizing training by Greene (167) and a shorter article (168) by the same author; a volume on training sales employees by Burlton (80) and articles on apprentice training by Woodruff (470), on training in a chocolate factory by Payne (305), and on training foremen.

C. *Monotony*

The influence of *monotony* upon the efficiency and well being of workers in industry is treated in a number of the articles which have appeared in recent years. The factors conducive to boredom in industry (including specialization, methods of bringing material to workers, individual differences in intelligence, variations in the estimate of time) and methods for reducing boredom in industry are discussed in a series of broad generalizations by Wyatt (471, 472).

The "total situation" is again stressed in a brief discussion of monotony by Mayo (266).

An experimental study of a repetitive process is reported by Wyatt (473), who gives a general intelligence test, a series of manual tests of the analytic type, and a miniature test to 30 female wrappers in a soap factory. No significant relationships appear between any of these and an output criterion. Questioning of the girls and casual observations lead Wyatt to conclude that the dislike for the work is the feature of boredom and that it may be caused by lifting of weights required on the job, by an unequal distribution of supplies, by posture, and by the nature of the machine.

In a still later investigation by Wyatt, Fraser and Stock (475), we find a feeling of boredom fairly prevailing in 49 factory workers, (including filament winding, soap wrapping, etc.) only 13 of whom seldom feel bored. An association is noted by the authors between boredom and reduced work, fluctuation of production as reflected in changes in the shape of the work curve, overestimation of time intervals, intelligence, system of wage payment, etc. They also discuss changes in methods of work which may result in a decrease of the feeling of boredom. The subjectivity of the "monotony" criterion must be carefully weighed in evaluating the findings of this study.

The work of other investigators in the field of monotony is reviewed by Thompson (432), who criticizes the failure of Wyatt and of Burnett to differentiate clearly between "efficiency" and "freedom from susceptibility to monotony". The author uses as a criterion of susceptibility to monotony a questionnaire on daily habits, in the form of a graphic rating scale, filled in both by the subject and by others acquainted with him for from 1 to 3 years. He finds a correlation of $+0.71$ between the criterion and a battery of four measures, including an emotional history record, "production decrement" on two performance tests and the score on an intelligence test. The outstanding weakness of this study is likewise in the subjective character of the criterion of monotony.

A laboratory study of one subject by Pollock (316), involving a long and short spell of work in pushing a small ball by means of a knitting needle, past obstacles, up an incline, leads to findings and conclusions of significance from the viewpoint of the problem of monotony in industry.

An interesting approach to the examination of susceptibility to monotony is embodied in an investigation by Lossag (254), who

examines differences in time estimation, by subjects (a) while looking at a blank wall, (b) while engaged in performing a simple automatic operation, and (c) while engaged upon a complex operation. The results lead to tentative conclusions on the applicability of this method in selecting for repetitive work those who are least susceptible to monotony.

Music is suggested as a counter irritant to the feeling of monotony by Younger (479).

D. Motivation

An increasing interest in the motivation of the worker—his attitude, feeling tones, and in the development of experimental techniques for their studies is reflected in the literature of the last few years. The influence of aptitude for work, training, conditions of work, rest pauses, regularity of employment, etc., upon the "will to work" is discussed in a common sense application, by Miles (276), of psychological findings to the problem of industrial motivation. The importance of evaluating scientific personnel procedures in terms of workers' satisfaction, as well as in terms of efficiency, is stressed by Kornhauser (219) in recommendations for the study of worker's feelings.

Significant findings on workers' attitudes grow out of an extensive and unusually well controlled investigation reported in a series of articles by Pennock (310, 311), and by Putnam (311, 325). The effect of the introduction of changes in wage payment, rest pauses, illumination—varied one at a time, is studied. An increase of production resulting regardless of the character of the change is explained by the author in terms of changes in mental attitude paralleling the variation in conditions of worker. This finding on the significance of mental attitudes in production leads to a direct study of workers' attitudes by means of confidential interviews, in which the worker is allowed an opportunity to give free expression to his opinions on every phase of plant operation. The importance of the information so obtained in developing a sound employees' relation policy, and in the adjustment of individual workers, is stressed. A by-product of this study is a supervisory training program in which information on employees' attitudes is employed.

Similarly important from the viewpoint of objective methodology in the study of employees' attitudes are the procedures adopted by Houser (194) in an investigation of what the employee thinks. The

analysis of trends of thought follows directly from answers to specific questionnaires.

In contrast to this direct approach of employe attitudes is the method employed by Lossagk (256), who studies the attitudes of employes toward their work by sending questionnaires to foremen. The investigation is worthy of note by reason of the naiveté of its procedure.

A study of emotional tones and attitudes of 12 male workers by Hersey (180) reveals a cyclical change from "happy" to "worried" apparently associated with such factors as changes in the blood pressure, feelings of fatigue, etc. The approach is interesting, but only the most tentative conclusions can be drawn because of the number of cases, the subjective character of ratings on emotional tone, and by reason of liberties taken by the author in completing curves.

An attempt to analyze workers' attitudes, with particular reference to the introduction of machinery, is included in an investigation by a sub-committee of the German Industrial Inquiry, described by Lippman (252, 32). Generalizations on the attitude of workers toward the introduction of machinery are included in an article by Davies (101). Fox (137) becomes emotionally upset at the machine's domination of man, in a criticism of industrial engineering. DeMan (102) investigates the attitude of workers to machines, by means of a questionnaire to 78 members of the Akademie der Arbeit of Frankfort. The limitations of this method are recognized by the author.

Jones (202) refers to the operation of "mental inertia" or perseveration in the genesis of attitudes of workers.

A consideration of workers' attitudes toward research procedures is urged by Poppelreuter (319), who describes an instance of objection on the part of workers to submit themselves to psychological examinations. The author concludes that the psychologist should, in so far as possible, avoid imposing tests against workers' wills, although, as he points out, this has been rarely necessary.

The effect of *financial incentives* is studied in other investigations. Ketzer (208) compares day and piece systems of payment in the case of a "good", a "fair", and a "poor" packer and finds that with a piece wage there is an increase in average daily production and a disappearance of the individual form of the work curve. Vernon (443) finds that a bonus tends to increase speed in work and to over-fatigue unless an adjustment is made in working. The beneficial effects of financial incentives on the attitudes of workers

is discussed by Schomerus (355). In the consideration of financial incentives the industrial psychologist will be interested in a description of wage incentives and their administration by Lytle (260).

E. *Maladjustment*

Closely related to the study of workers' attitudes and motives is an analysis of factors determining the *maladjustment* of individual workers in industry. The literature of the last few years includes a number of papers reflecting the point of view that vocational adjustment is a dynamic problem, involving a careful analysis of employees at work, and the readjustment of the employed worker, as a supplement to the scientific selection of competent personnel. This outlook finds expression in an article by Anderson (10) proposing a staff consisting of a psychiatrist, a psychologist, and a psychiatric social worker to handle problems of worker maladjustment. The actual operation of such a plan is described in his volume on psychology in industry and in articles by Dixon (111), Ray (336), Laughead (240).

Toulouse (434) uses results obtained from the examination of patients in a mental hygiene clinic to estimate the loss in earnings to workers suffering from mild psychopathic disturbances, and expresses the opinion that the total loss amounts to hundreds of millions of francs. He points out the usefulness of the psychiatric examination, supplementing the psychometric examination of aptitudes, as a means of decreasing the incidence of such psychopathic disturbances in industry. The significance of mild psychopathic disorder finds striking confirmation in the findings of Smith and Culpin (379) on the heightened degree of susceptibility to telegraphist's cramp of "neurotics" in this occupation. The elimination of psychoneurotics from this occupation by the use of validated selection techniques is recommended by the authors.

Culpin (99) discusses the incidence of "nervousness" in industry. Mayo (267) stresses the consideration of the whole individual and his adjustment as the proper subject of investigation in the promotion of human efficiency in industry. Programs for promoting the adjustment of the feeble-minded in industry are described by Burr (81), Raymond (327), and Bernstein (58).

F. *Fatigue*

Although appearing incidentally in a great many studies on conditions of work, *fatigue* as a specific psychological problem appears in but few publications. In general, the approach is physiological

rather than psychological. A few titles representative of the latter approach in the study of industrial fatigue are briefly summarized below.

The emphasis on underlying physiological factors is well illustrated in a volume edited by Atzler (40), embodying contributions from leading German investigators in the fields of anatomy, physiology, the measurement of fatigue, etc. Of particular interest to the psychologist are the chapters on the theory of fatigue and the discussion of its measurement, and chapters on fatigue in industry. An excellent bibliography accompanies each chapter. In the same category is another much shorter collection of papers on fatigue (54). The problem of objective measurement of industrial fatigue occupies primary position in this survey of work in fatigue, although little attention is given to psychological methods for its measurement.

The influence of conditions of work is reported in many studies. Of historical interest is a description of an early investigation by Imbert (198) of changes in respiration, circulation, and other indices of fatigue in the transportation of bread on a pushcart, undertaken to settle a disagreement between bakers and porters concerning the weight of a reasonable load. Paralleling this a more recent investigation of the physiological cost of muscular movement in transporting bricks on a barrow, by Crowden (97).

The effect on oxygen consumption, pulse rate, blood pressure, etc., of different methods of carrying load is studied in another investigation (86). Laboratory and plant investigations on the effect on fatigue of length and shift (445), of rest pauses (476, 445, 446, 262), of high temperature (441), of speed of work (274), of variety and uniformity of work (474) are discussed in a number of publications.

The harmful effect of noise, from the viewpoint of fatigue, as measured by changes in production in typing and by a greater expenditure of energy, is discussed by Laird (237). Results somewhat contradictory to these findings on the effect of noise are reported by Kornhauser (220). However, differences in the conditions of the two experiments must be noted.

The significance of physical factors in work and fatigue is investigated in a comprehensive study of physical characteristics of women in industry as compared with those of control groups (87). The effect of eye strain is considered in other studies (467, 468) Hill (185) contributes a treatise on recent additions to the knowledge of physical and chemical factors in recovery from fatigue in muscular movements of men.

Of more direct psychological interest is the explanation by Lewin (246) of fatigue and of monotony in terms of psychic satiety.

Among laboratory investigations of significance from the viewpoint of fatigue is Neifeld and Poffenberger's (291) treatment of Joteyko's expression of the curve of work, Weinland's (464) study of variability of performance in the curve of work, and Flügel's (135) results in a study on practice, fatigue, and oscillation.

Goldstern (164) contributes a conscientious study of the influence of fatigue as determined by errors in exact measurements by use of the polarimeter by 20 subjects. The article includes a lengthy bibliography. Verwoerd (448) questions the suitability of the "attention and fatigue meter" of Piorkowski as a measure of fatigue in tasks involving distribution of attention on the basis of the findings in an experiment with this instrument.

The effect of rhythm on fatigue is discussed by Sachsenberg (346) on the grounds of an investigation in a cigarette factory in which the introduction of a suitable rhythm of work was followed by an increase in the number of boxes packed and by decreased fatigue.

Rupp (345) contributes an analysis, in subjective terms, of psychological factors affecting production where the moving belt is employed, and of the effect upon the fatigue and attitudes of workers of this innovation. Experimental results are cited by Dukin (116) to demonstrate the lowered fatigue of an imposed rhythm of work. The author draws conclusions therefrom upon the desirability of the use of the moving belt in industry. Lehmann (244), by the use of the ergograph and of sustained weights, measures the cost of dynamic versus static work and finds the former to be more costly. He also contends that simultaneous work with both hands results in less production than the successive manipulation of two hands.

A review of laboratory investigations bearing upon the measurement of fatigue by the respiratory method is contributed by Neoussiki (292). Dean (101a) publishes a comprehensive bibliography on fatigue and allied subjectives.

G. Management

A number of books and articles are devoted to the psychology of management. In a volume by Metcalf (273) are included a series of excellent papers by psychologists on the application of psychology to various aspects of management in industry. A very general and nontechnical treatment of such factors as habit, the forces of per-

sonality, problems of self-control, etc., as applied to the problems of supervising workers is contributed by Smith (377) in a treatise on psychology for executives. A final chapter is devoted to the concept of integration in management as modeled after Follett. A somewhat similar approach is embodied in a volume by Tead (427), likewise given over to a general, nontechnical treatment of psychology as related to management. Although, as is true of other books in this field, tending toward wide generalizations, both volumes embody a good deal of material of value to the executive.

Chapters on the psychology of leadership and management are included in a dissertation by Swift (423) on how to influence men. There is a tone of finality in the recommendations and conclusions which hardly seems in place in a field in which there are practically no experimental findings upon the basis of which to formulate conclusive generalizations. The same subject is treated in a reprinting of a volume of Gowin (165).

The relative merits of argument and suggestion, emphasizing the greater suitability of the former in promoting management and sales activities, is discussed by Scott (366) in a new edition of an early work. The general psychological problems of leadership are discussed by Carrard (365). Achilles (2), and Tramm (436) contribute shorter articles on this topic.

H. *Miscellaneous*

A few additional contributions, *miscellaneous* in so far as character is concerned, remain to be mentioned. Giese (168) makes an unusual contribution to the study of physiological and psychological factors in work in a volume describing investigations on the hand as an instrument of work. A wealth of material on racial and sex differences in hands, on the hand as an agency for expressing subjective states, the pathology of the working hand, as well as upon the hand movements and forms in relation to occupational activity is assembled in this book. Numerous pictures, diagrams and bibliographical references add to the completeness of this volume.

The catholicity of interest on the part of European industrial psychologists is shown in an experimental study of success and failure in games of chance of the slot machine variety by Roloff (334) in order to determine the part played by skill. Results are related to legal definition of games of chance. Poppelreuter (321) contributes a splendid introduction to psychological factors in time study in the form of forty-six principles, illustrated with findings from laboratory

and factory investigations. Gardiner (147-151) publishes a series of articles of little merit on the influence of home conditions upon industrial efficiency.

The effects of ultra violet rays on industrial output involving eleven girls, three of whom constitute a control group, is investigated in an experiment by Punch, Wilkinson, Brooke and Myers (324). A careful control of conditions is supplemented by judicious interpretation of differences in effect produced in the case of the eleven subjects. An experiment by Sowton, Myers, and Bedale (386) is designed to examine the influence of menstrual cycle on bodily and mental efficiency. A series of tests given for a period of from four to six months to thirteen university women and to sixteen factory women ignorant of the object of the tests gave no evidence that the menstrual period in normal women is associated with serious incapacity for mental or muscular work. Observations on one subject on ninety-two consecutive days under controlled conditions of exercise, diet and sleep gave the same results.

Wiechman (462) suggests the desirability for unification of terminology in industrial psychology. He includes with his plea a short German vocabulary of many words used in the same sense in the literature.

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NOTES AND NEWS

Dr. John A. McGeoch, formerly professor of psychology in the University of Arkansas, has been appointed professor of psychology and chairman of the department of psychology in the University of Missouri.

Dr. Addison W. Moore, emeritus professor of philosophy at the University of Chicago, died in London on August 25th. Early in his career Dr. Moore contributed to experimental psychology.

In honor of Dr. George F. Arps, professor of psychology and dean of the College of Education of the Ohio State University, a dinner was given on the tenth anniversary of his deanship. Dr. W. O. Thompson, president emeritus; Julius F. Stone, chairman of the board of trustees; Dr. Boyd H. Bode, and Professor Wilbur H. Siebert, paid tribute to him.

Professor Sigmund Freud, psychiatrist, of Vienna, has been awarded the Goethe prize for scientific and literary distinction.

THE PSYCHOLOGICAL BULLETIN

TENTH MEETING OF THE WESTERN PSYCHOLOGICAL ASSOCIATION, JUNE 13 AND 14, 1930

Report of the Secretary, PAUL R. FARNSWORTH, Stanford University

Three of the sessions of the Western Psychological Association were held at the University of Southern California. The fourth was held on the new campus of the University of California at Los Angeles. Approximately one hundred fifty were present. The address of the retiring president, J. E. Coover, was entitled "Is Psychology a Science?"

The following officers for the year 1930-31 were elected: Stevenson Smith, University of Washington, President; George H. Mount, University of Southern California, Vice-President; and Paul R. Farnsworth, Stanford University, Secretary-Treasurer.

The 1931 meeting will be held at the University of Oregon, probably the latter part of June.

The Emotionality of Sixty Infants During the First Year of Life as Evidenced by Their Crying in a Given Situation. NANCY BAYLEY, Institute of Child Welfare, University of California.

Sixty babies were observed once each month for a sixty-minute period, during which mental and motor tests, anthropometric measurements and photographs were made. A record was kept of the amount of crying and the situations under which crying occurred. Also the nature and cause of the crying, its intensity and continuity were judged by the experimenters.

With this material comparisons have been made between different ages to determine the influence of age on the amount, intensity and causes of crying. Correlations have been computed to determine the consistency in the form and amount of crying in the course of routine laboratory experiments.

Psychology in Medical Education. J. W. BRIDGES, McGill University.

This paper deals with the relation of psychology to the other medical subjects, its place in the medical curriculum, and the content of a course for medical students. It is held that psychology belongs with the basic medical sciences rather than with the clinical subjects. Hence, instruction in the science is a task of the psychologist, while training in its application is the duty of the clinical instructor, just as in the case of anatomy, physiology and pathology. This presupposes that the clinical instructor is as conversant with psychology as with the other medical sciences.

Psychology is more difficult than anatomy or physiology because its subject matter is more complex. It should therefore follow these subjects in the medical curriculum. It will thus become more comprehensible to the student, more relevant to his other studies, and more effective in his clinical experience. Psychology should of course precede psychiatry as well as other clinical subjects.

The course should include a consideration of both normal and abnormal behavior and consciousness; and the psychological facts and theories should be linked as closely as possible with those of physiology. Moreover, the standpoint should be broad and eclectic, for the truth is more likely to be found in a reconciliation of views than in the prejudiced opinions of extremists. The main object of the course is to give the student a point of view, an understanding of the fundamental problems, and some knowledge of the well-established facts and principles of psychology.

Some Observations on the Social Development of Pre-School Children. K. M. BANHAM BRIDGES, McGill University.

Social development may be said to consist in the gradual development of more and more adequate and socially acceptable responses to social situations.

A three-year study of the social behavior of nursery school children revealed certain roughly defined stages in preschool social development.

There are seven or eight stages in group cohesion and group play noticeable between the ages of two and five years. There are three or four stages in personal contact with other children, three stages in altruism or other-mindedness, and three stages in the socialization of language.

During this same period there are also four distinct phases of

development in a child's relations with the adult. These are: dependence under two years of age, growing independence between two and two and a half years, negativism from two and a half to three and a half years, and coöperative independence over four years of age.

An Experimental Study of the Relation of Physical Resemblance to Mental Resemblance in Sibling Pairs. BARBARA S. BURKS, Stanford University, and RUTH SHERMAN TOLMAN, Occidental.

The problem is to ascertain whether or not siblings whose heredity appears to be unusually similar by the criterion of physical resemblance are more alike in mental endowment than siblings whose physical resemblance is slight. The sibling groups compared were selected from the Pasadena public schools on the basis of physical resemblance rated upon a 9-point rating scale. Independent ratings assigned to unselected pairs by the two experimenters agreed to the extent of a correlation of .67.

In grades 7 to 12, 84 pairs of siblings were tested upon the Terman Group Test. The I.Q.'s of the siblings who rated in the top half for physical resemblance gave a correlation of .67, those of the remaining siblings a correlation of .61. These correlations approximate those reported by Thorndike for group tests of siblings attending high school. The Stanford-Binet I.Q.'s of a group of 34 pairs of siblings between ages five and thirteen inclusive, whose physical resemblance was very strong, gave a correlation of .45. This correlation is close to those reported in studies of elementary school siblings tested on individual tests. Thus no association between general physical resemblance and mental resemblance is found.

Individual Differences in the Speed of Serial Reaction of Large Muscle Groups. MALCOLM CAMPBELL, University of Oregon. (Introduced by R. H. Seashore.)

The production of an apparatus permitting serial reaction with equal ease from any major portion of the body preceded the comparison of four musculatures, the two arms and legs, as to speed of serial reaction. Fifty college sophomore men acted as observers.

Reliabilities for the tests ranged from $r_{\frac{1}{2}-\frac{1}{2}}=.93$ to .96. Intercorrelations ranged from $r=.84$ to $r=.96$. Comparison with a test for speed of serial reaction with the fingers gave an intercorrelation of .65. Correlations with intelligence test scores, ratings on training, endurance in gymnasium tests, height and weight were insignificant.

The presence of a general intermusculature determinant is indi-

cated by the results. A proposal is made concerning the general or specific nature of motor ability and some suggestion is included concerning the trend of future investigation in the light of present results.

The Bogey of the Personal Equation in Ratings. HERBERT A. CONRAD, Institute of Child Welfare, University of California. (Introduced by H. E. Jones.)

In an effort to minimize the "personal equation" in ratings of intelligence, the Army psychologists made adjustments on the basis of the normal curve. Calculation shows, however, that the adjusted ratings correlate no higher with the criterion of test scores than the unadjusted ratings.

This led the writer to an experiment in which the adjustments to the raw ratings could be made on a thoroughly empirical basis; and in which the disturbing effect of the personal equation was designed to approach a maximum. In this experiment, the mean (also median) personal equation between teachers in the same nursery school amounts to a lowering of correlation coefficients by only .013; the mean personal equation between teachers in different schools produces a lowering of correlation coefficients by only .022 (median .019).

The conclusion is therefore drawn that ratings are blemished by the personal equation to an extent far less than has been supposed. Attention is thus fortunately freed for concentration upon the elimination of other shortcomings in ratings. It may be hoped that when the rating technique has been as carefully studied and refined as the testing technique, results from ratings will be improved to the point of scientific, as well as clinical, value.

Further Results of Analysis of Expert Typing. J. E. COOVER, Stanford University.

I have already shown that sequences of strokes in typing may be divided into five finger techniques and that these require of the expert variable times in a range amounting to the ratio of 1:3. Correlation between experts was as high as .90, showing the laws to be general. I present here evidence that the same laws may hold for the amateur typist, when copy consists of either a familiar speed sentence, or new matter. A new analysis of the typing of another expert reveals the sway of the same laws; and also two individual differences which, when removed by intelligent drill, will increase her rate of typing 17 per cent. These individual differences are

magnified in a speed sentence which permits a rate of 170 words per minute. The data from this same phrase show the persistence of a high skill over a period of 15 years, during which no consistent drill was taken. The loss in skill was but 4 per cent. Finally, the times of carriage returns obtained from an expert who is equally practiced with left-hand and right-hand levers, show that the left-hand lever requires about 21 per cent more time in typing English than the right-hand lever.

The Influence of Letter-Position on Range of Apprehension. H. R. CROSLAND, University of Oregon.

One hundred twenty-five beginning psychology student subjects, in the range of attention experiment, where a gravity tachistoscope presents for 100 σ nonsense arrangements of 18-point, Caslon Bold capitals, in series of 4 through 9 letters per card, 10 cards per series, give results with a reliability coefficient of 0.7 or better. The results, when graphically depicted, reveal a descending effectiveness of letter-position from the left toward the right, regardless of shortness of exposure-series, regardless of the composite method of graphing, and regardless of the percentage frequencies with which the cards were reproduced without error. Faultless reproduction of cards occurred for only 52 per cent of the 4-letter series, 19.7 per cent of the 5's, 2.3 per cent of the 6's, 0.3 per cent of the 7's, 0.0 per cent of the 8's, and 0.0 per cent of the 9's. Graphs depict the foregoing percentage frequencies, the composite frequencies for letter-positions in Crosland's and Johnson's earlier work, composite letter-position frequencies in this work, the letter-position frequencies in detail for each series, and for the letter-position frequencies in cards containing only one or more errors (that is, in records from which faultlessly recalled cards have been excluded). There is a decided tendency in each series and for the letter-position frequencies in cards containing only frequency than its immediately adjacent neighbor to the left. The longer the series the more does this tendency apply to the next to last and to the second from last characters from right to left on the exposure cards.

A Forward-Going Tendency in Maze Running. J. F. DASHIELL, University of North Carolina.

(1) In a multiple-U maze white rats were found to run the true path more easily when the blinds were located always on the same side (C) than when they were located alternately right and left (A).

This phenomenon was examined by variations of mazes in several series of experiments. (2) When additional segments were added to the multiple-U mazes the animals manifested a tendency to run in the C manner regardless of which way they had been trained in the original maze. (3) When the C and A mazes were built on scales both smaller and larger, no alteration was found in the fact of easier running in the former. (4) Motion picture analyses brought out little if any evidence for any maintenance of gross orientation from turn to turn. (5) Simple mazes consisting of a straightaway, an elbow, and a T-choice with one alley running forward and one backward, when used with untrained rats brought out some indication of tendency to choose the former type of alley.

Different hypothetical explanations of the greater easiness of the C type maze are canvassed. It seems likely that the factor most responsible is a forward-going tendency in animal locomotion that lends not only maintenance for short distances of a direction already set but also to a compensatory sort of correction when forced out of line by an obstruction.

Imagery Differences and Their Functional Significance. F. C. DAVIS,
University of California. (Introduced by Warner Brown.)

The assumption is often made that auditory imagery plays an important rôle in musical accomplishment, that visual images function in the work of landscape and portrait painters, and that in the field of literature a preference for images of a given modality is discernible in the work of certain writers and in the judgments of critics. The present investigation was planned to show what differences, if any, can be found among university students in the kinds of imagery reported by them as functioning in particular test situations; and given such differences in the subjects' reports, to determine what significant differences, if any, obtain between the test scores of those reporting different processes. If such differences are found, with subjects of such relatively homogeneous background and intellectual capacity as state university sophomores and juniors, then some weight would seem to be added in support of the hypothesis that imagery differences are of functional importance in such highly specialized pursuits as the several arts seem to be. In this experiment significant differences are found, in certain of the tests designed to favor the use of a given kind of imagery, between the scores of those reporting that kind of imagery dominant and the scores of those reporting it weak or absent. The hypothesis that the tests tap different func-

tions rather than some general capacity like "immediate memory" is supported by the subjects' reports and by the relatively low inter-correlations of the various tests.

A Criticism of Unilateral Cerebral Dominance as an Explanation of Word Blindness. GRACE M. FERNALD, University of California at Los Angeles.

Criticism of Orton's theory from the point of view of brain function and from the experimental results with over 300 cases of word blindness and allied phenomena.

Meaning and Insight in Köhler's Psychology. S. C. FISHER, University of California at Los Angeles.

Naïve experience—the "layman's consciousness"—is by no means as replete with "why's" and "how's"—*i.e.*, *insights*—as Köhler maintains. Rather, these "insights" are present as reflections or after-thoughts when some situation elicits tendencies to interpret or communicate the meaning of experience. In support of this statement, we refer to certain by-products of a series of experiments in which observers were engaged in describing the experiences present in connection with accepting or rejecting novel presentations as members of a previously studied group. (Psychological Monographs, Vol. XXI, esp. pp. 175-179; *Amer. Jour. of Psychology*, XXVIII, esp. p. 98.) Observers reported the continuum of events; the perceived figures or features behaved in immediate experience as general or non-general, as belonging or not belonging to the group. Kinaesthetic or affective attitudes functioning as acceptance or rejection were often present. But the meanings or "insights" which in Köhler's view might have been expected—awareness, *e.g.*, that a feature was noted *because* it was general, etc.—were comparatively rare in the subject's experience, dominated as the latter was by a vigorous "determining tendency" to define or classify. When it did occur the "meaning" or "insight" was apparently a verbal label for a reflective set induced by a number of conditions. Among these conditions figured prominently such factors as need of social communication of the import or meaning of a given experience, theoretical interest or speculation on the observer's part as to the object of the experiments, or especially conspicuous accompanying contents. In the more mechanized procedures, observers never reported such "insights." It is possible that concern with the dynamical aspects of the physical *Gestalt* as hypothecated by Köhler would serve as a

condition tending to elicit reflective states which would be referred back as "insights" into the direct continuum of experience. But the assumption that "insight" is a characteristic and very common component of unreflective human experience seems unwarranted by any evidence on hand.

Experiments in Vision and Touch. SHEPARD I. FRANZ, University of California at Los Angeles.

Reports will be made of certain improvements in vision and touch accompanying training, and a discussion of the facts in relation to possible cerebral activities.

Experiments on Maximum and Minimum Action in the White Rat and Least Effort and Insight in Monkeys. J. A. GENDERELLI, University of California at Los Angeles.

A series of experiments was performed with blinded and normal white rats to determine the nature of the path which the animals would eventually select from an indefinite number of possible paths leading to food. It was found in practically all cases that the path finally chosen by the animals was the path of least distance.

A problem box was constructed and stylus mazes used for the purpose of investigating the tendencies of least effort and insight in monkeys.

Anticipations of Perception in Color-Mixture. KATE GORDON, University of California at Los Angeles.

British empirical psychologists had taught that we cannot frame in our minds a sensory element which has not come to us from sense, but Hume allowed a certain exception in that passage about the shades of blue, where he supposes that a man who had seen all but one in a series might perhaps imagine, from those on either side in the series, what it was like. Color discs, mounted on a wheel, were shown to a group of 103 persons. On each disc were two or more colors. The subjects were asked to try to imagine what these would look like when they were mixed by rotation. In order to have some way of recording their guesses as to the color, a chart of samples was presented and the subject designated the sample which he thought would match the resultant color. The wheel was then turned, and the subject looked at the mixed color and matched it to one of the samples on the chart. In this way the discrepancy between the guessed color and the perceived color could be described in terms of the number of

intervening color samples. The samples used were 100 shades of the Munsell series which vary by small steps from one end of the spectrum to the other, and are stated to be of equal saturation. Results show that when two colors near each other in the spectrum are mixed it is possible to guess closely, but that it is possible to find near complementaries which will make the guesses resemble mere chance.

A Comparison of the Learning Ability of Mus Norvegicus Albinus and Mus Norvegicus on Alley and Elevated Mazes of Identical Pattern. HARRY F. HARLOW, Stanford University. (Introduced by W. R. Miles.)

A comparison was made of the learning ability of domestic albino rats with the immediate descendants of wild brown rats on both alley and elevated mazes. The mazes themselves were identical in pattern, presented fifteen blind alleys, and had reliabilities (odd vs. even runs) of about .70. The correlation between the mazes for the albino rats was in the neighborhood of .70 and for the brown rats about .40. The albino rats were superior on both mazes by all three criteria chosen, time, errors, and trials. For both mazes the difference of the means divided by the probable error of the difference was about three for trials and errors and over ten for time. The alley maze proved to be the simpler of the two. The differences in the error scores, however, failed to prove significant for either group of rats, and trials were significant only for the albinos. Rating the brown rats for wildness by Yerkes' criteria and comparing the scores made by wild and tame members of this group with the average score made by the albinos, indicated that the differences shown by the two kinds of rats were caused by emotional traits and not learning ability.

The Belief in Attitudes. M. E. HERRIOTT, Los Angeles City Schools.

Attitudes are almost universally recognized as potent determining factors of behavior. Laymen, teachers, educationists, and psychologists may be quoted to this effect *ad infinitum*. Evidence of this recognition may readily be found in the daily press or one's daily contacts and in the writings of such educationists as Bagley, Bobbitt, Book, Briggs, Cameron, Clement, Colvin, Dewey, Inglis, Judd, Monroe, Ruediger, and Thorndike.

Despite the prominence of behavioristic and mechanistic psychologies, one need not enter into a lengthy discussion in order to establish

the fact that psychologists generally recognize attitudes. Even John B. Watson can be quoted in their support.

Dr. Ellen B. Sullivan has presented an outstandingly excellent summary of psychological investigations of attitudes.

Such psychologists as Titchener, Stout, Angell, Marbe, Mayer, Orth, Ach, Binet, Bühler, Müller-Freienfels, Grünbaum, and Pillsbury are quoted freely. I need not give specific citations here.

In 1922 a committee of the American Psychological Association under the chairmanship of Warren formulated four definitions of mental attitude.

Four years ago the writer undertook a study of attitudes as factors of scholastic success. In this investigation he attempted to get at the following factors: previous preparation, intelligence, reading ability, study habits, the ambitious-indifferent attitude, the cheerful-despondent attitude, the evaluative-nonevaluative attitude, the persevering-vacillating attitude, and the self-confident-dependent attitude. Certain conclusions were, of course, arrived at which may or may not be valid. Of first importance was the conclusion that the five major factors of scholastic success are: (1) previous preparation, (2) intelligence, (3) study habits, (4) evaluative-nonevaluative attitude, and (5) persevering-vacillating attitude, of which the last two, as judged by the size of tenth-order partial coefficients of correlation, are the most influential.

But more important than the direct outcome, it seems to the writer, were those incidental to the study. Three fundamental gaps in the science of education were made almost painfully evident. In the first place, there is no adequate knowledge or generally accepted theory as to the nature of attitudes and related traits or characteristics. In the second place, we do not have adequate measuring instruments for all aspects of mental life. So-called intelligence tests and educational tests serve reasonably well, but we do not have a firm foundation upon which to stand even in these fields. In the third place, we have no developed statistical techniques that are wholly adequate to our needs, nor have most researchers become fully aware of the uses and limitations of those techniques at present available.

Delayed Reaction in Rats. C. H. HONZIK, University of California.
(Introduced by E. C. Tolman.)

Rats were trained to choose the one door in three which contained a white curtain, and not to try the other two doors which contained black curtains. When this discrimination habit was firmly estab-

lished, the rats were allowed to run toward the white curtain but not to pass through. A screen which obscured all the curtains was immediately lowered, the rats were delayed for varying periods of time, and during the delay period a black curtain was substituted for the white. When the screen was raised the rat was confronted with three black curtains and he had to choose that door which had contained the white curtain, that door being the only correct one. Positive results were obtained.

The Galvanic Skin Reflex in Young Children. H. E. JONES, Institute of Child Welfare, University of California.

Galvanometric records were obtained from 54 preschool children, tested on two occasions by a standardized series of emotionally provocative stimuli. Results were analyzed in terms of: (a) average deflections for each stimulus; (b) average latent periods for each stimulus; (c) the effects of negative adaptation; (d) the reliability of galvanometric determinations, in terms of correlations of the first and second series; (e) the specificity of the galvanic skin reflex, in terms of intercorrelations of the deflections to the various stimuli; (f) the relationship of overt and implicit response; and (g) the validity of galvanometric data, as checked against other criteria of emotionality.

Some of the Functions Underlying the Responses of Monkeys to Stimuli in Different Sense Fields. HEINRICH KLÜVER, Institute for Juvenile Research, Chicago.

Groups of equivalent and nonequivalent stimuli were determined in various sense fields in a series of experiments conducted with Old World and New World monkeys (*cf.* H. Klüver, *The Equivalence of Stimuli in Monkeys. Proceedings and Papers of the Ninth International Congress of Psychology*, p. 263. The following facts seem to stand out:

(1) The properties of the stimulus objects may be changed radically without upsetting the response of the animal; *i.e.*, no matter how much the stimuli differ with respect to their properties the consistency in the reactions of the animal indicates that equivalent stimuli must be alike in terms of *one general characteristic*;

(2) Experimentally it is often found that this general characteristic is linked up with a definite property of the stimuli, *e.g.*, rhythm or relative size, a property which must be considered *crucial* so far as the reaction of the animal is concerned;

(3) Although the efficacy of some general characteristic must

be inferred from the consistency of the responses, it is not to be concluded that under nonexperimental conditions the animal reacts in terms of such general characteristics; it is merely demonstrated that the animal is able to fall back on such general characteristics. (Problem of *fragility* of functions);

(4) It is found that the same general characteristic is reacted to with greater ease in some stimulus situations than in others; although equivalent stimuli must be considered similar, there are *degrees of similarity* as indicated by the behavior of the animal (increase in number of successive trials, hesitation, etc.).

(5) The fact that very general characteristics may be primarily effective in determining the behavior of the animal under experimental conditions does not give us any information about the *genesis* of the responses, *e.g.*, on the question whether they are native or acquired.

In formulating a *working hypothesis* concerning basic functions in primate behavior the following points must be emphasized:

(1) Psychologically viewed, the responses of the animals are not determined by the objective properties, but by the phenomenal properties of the stimuli;

(2) "Peripheral" physiology showing the dependence of certain physiological events on certain stimulus conditions cannot at present throw any light on the basic functions considered here since it does not furnish any precise information on the physiological correlates of phenomenal properties;

(3) Since very general characteristics are not only effective in determining the behavior of monkeys, apes, and men, but also of bees (M. Hertz) and rats (Lashley) it is assumed that animals of different species are adequately characterized from a psychological point of view only if the following points are taken into consideration: (a) the number of stimuli the animal can react to in terms of one general characteristic; (b) the ability of the animal to differentiate the stimuli belonging to groups of equivalent stimuli; (c) the "fragility" of the reactions of the animal; (d) the dependence of these reactions on the neural, physiological, etc., equipment of the animal.

A Comparison of Time, Errors, Distance, Excess Time, and Speed as Measures of Maze Learning. J. R. LIGGETT, University of California at Los Angeles.

The study represents a statistical comparison of time, errors, distance, excess time, and speed scores obtained from 25 rats in a

sectional maze. It was found that the maze measures two factors (1) the elimination of errors or excess distance, (2) increase in speed. Error scores or distance scores give a measure of the first of these factors. The second factor is best measured by speed scores. Time scores are not accurate, because they are influenced by errors. The two learning factors were found to be independent (except that they are concurrent).

The Language Development of the Preschool Child. DOROTHEA MCCARTHY, California Bureau of Juvenile Research.

Fifty consecutive verbal responses were recorded for 140 children at each of seven year levels at six months intervals from 18 to 54 months. Paternal occupation was used as a criterion in order to secure a representative sampling of the population:

These data were subjected to four different types of analysis: (1) according to the length of response, (2) according to the complexity of sentence structure, (3) according to the function of the response based on a modification of Piaget's classification, and (4) a word analysis according to the various parts of speech. Each of these analyses was carried on in relation to age, sex, paternal occupation, mental age and age of associates.

The results yield tentative normative material on linguistic development during the preschool period. In addition to the developmental trends noted with increase in chronological age, there appeared small but consistent sex differences indicating earlier linguistic maturity among girls than among boys. These differences were found in all phases studied as were also significant differences between the upper and lower occupational groups, indicating clear superiority of the children of superior socio-economic status in language development.

(NOTE: This study has just been published by the University of Minnesota Press as Monograph No. 4 of the Institute of Child Welfare Monograph Series.)

Relearning in the White Rat. QUINN MCNEMAR, Stanford University. (Introduced by C. P. Stone.)

The data presented come as a by-product of another major study, hence not a systematic study of relearning; yet the results are of value as indicating the factors which must be considered in studies of retention in the rat. Fourteen groups of rats were required to learn the Stone multiple-T maze, and then relearn under varying conditions as to laboratory experience, age, and retention interval.

The results indicate that age is a possible factor which might affect relearning or retention. Some indication is given to the effect that the forgetting curve may be curvilinear similar to the Ebbinghaus curve for humans. The correlations of learning with relearning indicate that the maze is consistent even over long intervals of time. Plans are being formulated for a thorough study of the factors affecting the retentivity of the white rat.

Auditory Fusion in the Vibrato. MILTON METFESSEL, University of Southern California.

The vibrato in artistic voices consists of a variation in frequency of vibration, cyclic in nature, with its extent on the scale averaging about a half-step, and ranging from a tenth to over a whole step. Its average artistic rate is almost seven cycles per second, ranging from five and a half to eight and a half. A vocal trill ranges from three-quarters to two full steps.

The auditory organization of the vibrato, however, is not the same as the physical organization. There is no change of pitch in the vibrato, and it was determined by the use of synthetic vibratos played from siren discs that the successive frequency cycles in the physical vibrato fuse into a single, steady pitch. The successive sound wave fusion also produces a new quality in the tone (sonance), and a pulsing.

This effect might be called "tone-mixing" inasmuch as there are two notes on the sound wave which fuse into one in auditory experience. Roughly, the pitch heard in a half-step vibrato is near to half-way between the two physical notes when graphed in terms of tenths of a musical step. Individual differences are apparent, suggesting that an artist may be sharpening for one auditor and flattening for another on the same note.

In the region near high C, the pitch heard drops slightly from the half-way point. A trill in this region a whole step in extent has two localizable pitches about a third of a step apart, the upper being slightly above the half-way point between the two physical notes (or extremes of the cycle).

Suggestion in Pictures. I. MISUMI AND P. R. FARNSWORTH, Stanford University.

Farnsworth and Beaumont¹ have previously demonstrated that it is possible to influence the preference ratings of pictures to a con-

¹ FARNSWORTH, P. R., and BEAUMONT, H. Suggestions in Pictures. *J. of General Psychol.*, 2, 2 and 3, 1929, 362-366.

siderable extent by means of attached and suitably composed descriptive paragraphs.

In the present study a survey was made of student familiarity with artists' names. The four best known and four least known names were then attached to certain pictures. Each picture was rated by two groups of students. One group supposed the picture to have been painted by a well known artist; the other group supposed it to have been the work of an unknown painter. Consistent alterations in the preference ratings resulted, indicating that considerable suggestion had been present.

Some Extensions of Psychological Measurement into Industrial Problems. HERBERT POPENOE, Stanford University.

Buying behavior of the public with regard to department stores, automobiles, transportation by railroad, phones, shoes, cosmetics, use of gas and electricity, newspapers, radio, and many other industrial situations are associated with mental "sets" of extreme definiteness and persistence. It is possible to measure the mental "set" of each individual of a group, and to determine statistically the relative contribution of each identified causal element involved.

This has been accomplished through the construction and use (in the form of interviews) of batteries of test items. These are developed in a manner very similar to that used in psychological and educational tests. The work has been carried on in more than thirty industrial concerns, involving more than 150,000 interviews.

In each study determinations have been made of: (1) the extensity of occurrence of each causal element; (2) the intensity of influence of each causal element; (3) the average level of the mental "set" of the group.

Analysis has shown that these "sets" are largely determined by the past experience of the individual, and in part by such factors as geographical location, social and economic status, occupation, sex, race, and age. It has been demonstrated that these "sets" have high reliability, are very amenable to statistical analysis, and are causative determiners of socio-economic behavior.

Studies in Mental Fatigue. A. S. RAUBENHEIMER and Students, University of Southern California.

The study was an attempt to verify the experimental findings of Arai and Thorndike in regard to the effect of continuous mental work (12 hours) on mental efficiency in specific functions.

Three subjects were involved in carrying on substantially the experiment of Arai. In addition, the influence of sustained mental work on certain other mental functions and on fundamental conditions of metabolism was studied.

The Effect of Inanition on Learning Behavior in the White Rat.
FLOYD L. RUCH, Stanford University.

At the age of 30 days three groups of rats, equated for sex and for body weight, were subjected to the following dietary conditions: a maintenance group composed of 28 rats was held at body weight by appropriate limitation of the food supply; an *ad libitum* group containing 23 animals was given unlimited opportunity to feed; and a retarded group of 28 was held to a rate of growth which was 80 per cent of that for the *ad libitum* animals.

At the age of 50 days the maze trials were started. The maze employed was a modification of the Stone Multiple-T Maze. The significant characteristic of the maze employed in this experiment was the fact that it was filled with water in such a fashion that the animal could escape only by swimming through it to the escape platform. The animals were fed prior to the first maze trial on a given day thus eliminating the factor of differential hunger motivation in the experimental groups. One maze trial per day for the first four days was given. Following this two trials were given each day until a total of 30 trials had been reached.

Performance of the animals was scored in the following manner: total time in seconds; Type I Errors or errors made when the animal going in a forward direction entered a blind alley; and Type II Errors which consisted of a segment of the maze retraced.

The Pearson coefficients of correlation based on forward errors made in odd numbered blind alleys vs. even numbered forward errors are as follows: maintenance, .67; *ad libitum*, .76; and retarded, .25. These coefficients have been corrected by the Spearman-Brown formula for halved data.

The differences between the mean performance of the groups are stated in terms of the Standard Errors of the differences.

CRITICAL RATIOS

Score	$M_m - M_a$	$M_a - M_r$	$M_m - M_r$
Time.....	0.68	1.18	1.70
I Errors	2.00	0.48	2.56
II Errors	0.92	1.64	2.48

In view of the low magnitude of these critical ratios, these results should be regarded as merely suggestive. In so far as the differences between the maintenance and *ad libitum* groups are significant, they tend to confirm the results of Anderson and Smith.

Further Studies in Rhythmic Auditory Motor Coördinations.

ROBERT HOLMES SEASHORE, University of Oregon.

Previous work at the University of Iowa indicated that temporal precision of the right hand in following a simple repeated rhythmic pattern by tapping a telegraph key is related to scores made on a sensory rhythm test (C. E. Seashore, Columbia phonograph record, "Sense of Rhythm"). This sensory rhythm test, which involves judgments of "same" or "different" on successive pairs of patterns, is in turn related to the number of trials necessary to learn a series of more complex sensory rhythm patterns.

The present study compares the scores made on the motor rhythm test with various patterns and with various musculatures in order to determine the validity of the procedure of testing one musculature on one pattern, as a possible test of aptitude for musical performance.

The Aptitude Hypothesis in Motor Skills. SIGFRID SEASHORE, University of Oregon. (Introduced by R. H. Seashore.)

The six tests of the Stanford Motor Skills unit were used on 50 operators of high speed winding machines, working under optimal industrial conditions in the Jantzen Knitting Mills. This afforded a critical test of the hypothesis of motor aptitudes as determinants of individual differences in complex practical motor skills. In spite of high reliabilities of both the motor tests and the criterion, measures of piece rate under normal working conditions, there was practically no correlation between the separate motor tests or the battery as a whole and the criterion.

A control experiment showed further that short samplings of several minutes work of maximum speed on the criterion performance are not indicative of the normal rate on the same performance. An additional study not yet completed attempts to discover by slow motion camera analysis whether the normally slow workers accomplish their "spurt" performances by different and more wasteful or more tiring methods than the normally fast workers.

The motor test scores of this group are further compared as to specificity of skills and reliability with results from a university group, a high school group, and a group of ten year old grade students (all male observers).

Utrocular Discrimination. STEVENSON SMITH, University of Washington.

Local sign of a single retina is not a factor in discriminating which of the two eyes is stimulated. If such a discrimination is possible there must be diverse local sign of corresponding points on the two retinas. Capacity to make this discrimination was established by the use of an improved apparatus which afforded stimulation of either eye from various angles and of various intensities. In order to determine whether utrocular local sign is a function of iris and ciliary movement, these muscles were relaxed by the use of homatropin and cocaine. With such relaxation utrocular discrimination was still perfect. This points to the possibility that such discrimination is a function of the external eye muscles.

Observations on Wildness and Savageness in Rats of Mixed Strains.
(From the Institute for Juvenile Research, Chicago.) CALVIN P. STONE, Stanford University.

In this experiment the following strains were employed: trapped brown rats, first generation of wild stock reared in the laboratory, crosses between albinos and wild stock, crosses between albinos and half-breeds, yellow hooded animals, and albinos of the Wistar stock. Each group, excepting the trapped wild stock, consisted of from 20 to 40 individuals reared under similar conditions and without handling until the outset of the experiment or the age of four months.

In the first experiment each animal was rated for wildness and savageness on a 10 point scale for 10 consecutive days. The order of wildness and savageness of these groups was rated as follows: trapped wild adults, descendants of the wild stock reared in the laboratory, half-breeds, quarter-breeds and yellow hoods, and lastly the albinos of the Wistar stock. In experiment two, strength of the hiding response as balanced against hunger was tested daily for 10 days. The obtained rankings in this experiment were similar to those of the foregoing except that the quarter-breeds were slightly less prone to hide than the yellow hoods. Differences between descendants of the wild stock and half-breeds and likewise those between the half-breeds and pure albinos were statistically significant. In the early trials only were differences between the albinos and the quarter-breeds or the yellow hoods consistently significant. Experiment three consisted of maze learning. Despite a long period of taming, the adult wild animals with only a few exceptions refused to run the maze; also their descendants even after the taming involved in the

first and second experiment which lasted 20 days were too wild to run the maze. All other groups, however, had become sufficiently tame at the end of the second experiment to run the maze with only occasional evidence of timidity. No striking differences in time or error scores were obtained for these groups. Without systematic preliminary training, however, many of the half-breeds and a small per cent of the yellow hoods and quarter-breeds refused to run the maze. On the whole it is evident that the factor of wildness uncorrected by systematic taming is incongruous with meaningful tests of maze learning ability.

Preliminary Report of Activities of Mental Hygiene Clinic of Los Angeles County Department of Health. ELLEN B. SULLIVAN, University of California at Los Angeles.

Organization, types of cases handled, methods and procedures in this clinic, together with a discussion of certain experimental phases of the psychological work.

An Experimental Study in Control of the Vocal Vibrato. A. H. WAGNER, University of Southern California.

Vibrato in the voice has long been a subject of controversy among musicians. Tones of nine well-known operatic singers were selected as examples of satisfactory use of vibrato in both calm and agitated singing. These tones were reproduced through phonophotography and analyzed as to rate, extent and regularity of vibrato. Control tone phonophotographs were then taken of the voices of forty students with less desirable vibrato and a group having no vibrato and technique developed for the cure of these vocal shortcomings.

Conclusions reached:

(1) That production of vibrato can be taught to preadolescent boys and adults of both sexes.

(2) That refinements in rate and regularity can be made by "harnessing" the vibrato to rhythm.

(3) That refinements in extent can be made by giving attention to the proper action of the abdominal musculature having to do with controlled expiration.

The Validity of Serial Motor Tests for Predicting Proficiency in Typewriting. ROBERT Y. WALKER, University of Oregon. (Introduced by R. H. Seashore.)

Seventy first-semester typewriting students were tested on the six tests of the Stanford Motor Skills Unit to measure their proficiency

on a sampling of serial motor skills. After seven months training in typewriting the students were given the standard form of speed tests to measure their attained proficiency in typing. Two criteria were used to check against the scores made on motor tests: (1) the gross number of strokes made on the typing test, and (2) the "rate" of typing in words per minute after a heavy penalty of 51 strokes had been deducted for each error, following International Contest rules.

No significant relationship was found between either criterion and the composite score (equally weighted) of the motor tests although they averaged $r_{\frac{1}{2}-\frac{1}{2}}=.84\pm.02$ for reliability. The only individual motor test which gave a significant correlation with typewriting was speed of tapping a telegraph key: with gross number of strokes $r=.28\pm.09$; with "rate" $r=.62\pm.06$. This is somewhat unusual because the reliability of "rate" was $r=.63\pm.06$, while the reliability for gross strokes was $.89\pm.01$. The reliability for errors was only $r=.28\pm.06$.

Neuromuscular Development During the First Twelve Months of Life. LOTTA V. WOLFF, Institute of Child Welfare, University of California.

As a part of a developmental study at the Institute of Child Welfare, 60 normal full term infants have been examined monthly from birth to twelve months, with a cumulative record of the growth (or lapsing) of the following reflex functions: Moro, abdominal, Babinski, postural adjustment to head rotation, Landau, spinal, Brudzinsky, and responses to changed body position. A group of tests of other motor functions was also included. The report covers a description of the responses studied, the growth curves for each response, and tentative conclusions concerning the nature and course of neuromuscular development during infancy.

Handedness in Rats. JOSEPH G. YOSHIOKA, Institute for Juvenile Research, Chicago.

Two hundred rats were tested for handedness with a specially devised food-pan. Nine were ambidextrous; among the rest 96 were right handers, and 95 left handers. When the bones of fore limbs were cleaned and measured, it was found that the ambidextrous rats had equal right and left arms, but among 96 right handers 36 possessed right arms longer than the left, 60 having equal right and left arms. In no case had a right hander a right arm shorter than the left. Similarly 45 among 95 left handers had left arms

longer than the right, and 50 had equal right and left arms, but in no case had a left hander a left arm shorter than the right. It seems that a preferential use of one hand may have originated from a better development of that arm. This view is supported by the fact that the anatomical asymmetry was found more often among the young animals than among the old ones.

It is concluded that in a random sampling of laboratory rats a very few are likely to be ambidextrous, and the rest divide themselves about equally to right and left handers. A preferential use of one hand may have originated, in part at least, from a better development of that hand and arm.

PSYCHOLOGY IN 1929 AT THE INTERNATIONAL CONGRESS

BY KATHERINE ADAMS WILLIAMS

Radcliffe College

At the meeting of the Ninth International Congress of Psychology such a large number of papers were read that their classification affords a view of the interests and trends of modern psychology.

The *Proceedings* of the Congress¹ comprise 422 abstracts of papers, exclusive of those of which the title only is given. Because of the relatively large number of papers from this country, inferences to the state of psychology in the United States will be better founded than inferences to the state of psychology in any other country, all other countries, or the entire world. Table I shows the distribution of papers among the various countries.

The classification according to subject-matter, shown in Table II, indicates what topics are of most interest. The sub-heads of the classification are determined mainly by the demands of the material, but are also related to the schemes used by the Psychological Index and the Psychological Abstracts, and to the names of the symposia at which the papers were given at the Congress. Since many articles necessarily fall under two or more heads, there is a great deal of overlapping. The wide range of subjects makes it unprofitable to list the foreign contributions separately by country. *Tests and Measurements* is the general topic under which the largest number of papers fall, although *Applied Psychology* ranks first in number of foreign contributions. It is noteworthy that *Animal Psychology*, which ranks fifth so far as total number of papers is concerned, boasts only two foreign contributions, both of which are made by Russians and both of which deal with the subject of the conditioned reflex.

The classification according to method of treatment of the subject (Table III) shows the relative parts played by reflective analysis and by experiment and research. It also answers such questions as the following: Is there a tendency toward the use of objective

¹ *Proceedings of the Ninth International Congress of Psychology, 1930.*

methods of observation as compared with introspective methods? How large a part does observation in 'real life' situations play as compared with laboratory observations? The papers listed under *Traditional Method* are neither clearly objective nor strictly introspective. For instance, in the traditional type of laboratory experiment the subject is often not required to give any elaborate introspective account but is merely told to *do* something. The observations of the experimenter are perfectly objective but the behavior is usually, according to the instructions, a sign of a fact observable only introspectively. The class called *Traditional Method* includes cases in which the report or 'sign' was verbal; but where an analysis or description of the conscious state was essential to the experiment, the report falls in the class called *Introspective Method*. *Tests* do not fit any of these classes, for they bear quite as close a resemblance to the 'traditional' laboratory technique as to the objective method of investigation and they are given under conditions which are as much like situations of 'real life' as like laboratory conditions. They, also, therefore, constitute a class by themselves. Descriptions of apparatus which might be used for either objective or introspective results, and discussions of statistical procedure are omitted entirely from this classification. There is still, however, some overlapping.

The classification shown in Table IV casts light upon the relative predominance of the points of view of various systems. Does the main interest lie in facts of consciousness or in facts of behavior? If the latter, are the facts studied facts of gross behavior, or are they physiological facts? Here as in the preceding table there is a special class for those studies in which the interest was in the correlations between stimulus and conscious response but not in analysis of the conscious state *per se* and not in objectively observable behavior. Two other classes of report are almost invariably ambiguous. In the first place, studies of maladjustment and of pathological abnormality very often take their departure from observations of abnormality in behavior but interpret this behavioral abnormality as a manifestation of a disturbance in consciousness. Again, mental tests may be of interest as tests of conscious process or as tests of behavior. Finally, there are a great many reports which give no clue to the nature of the interest. Here, as in the preceding classificatory scheme, descriptions of apparatus, and discussions of statistical technique which might be used in the service of either sort of interest, are not included.

Table V supplements Table IV. It classifies the rather small number of papers which explicitly support any definite position with respect to psychological systems.

Tables IV and V do not give an entirely satisfactory picture of the trend of psychology in its theoretical and systematic aspects, even as they were exhibited at the Congress. The most serious distortion occurs in the case of *Gestalt* psychology. There is, to be sure, only one report (by Köhler) which is explicitly of that school. Even this one is not polemical. There are, however, numerous references to the school. There are also, no doubt, a considerable number of problems undertaken under its influence, but, since the writers of the abstracts make no such admissions, the number cannot even be estimated. Psychoanalysis is in a similar case. It is mentioned and discussed, more or less, and its method was undoubtedly used in some of the researches reported, but any precise determination of the number is impossible.

TABLE I
DISTRIBUTION OF PAPERS BY COUNTRIES

U. S. A.	321	Switzerland	2
Germany	26	China	1
Great Britain	17	Denmark	1
U. S. S. R.	13	France	1
Canada	6	Hungary	1
Holland	5	Japan	1
India	5	Lithuania	1
Italy	5	New Zealand	1
Austria	4	Norway	1
Belgium	3	Palestine	1
Argentina	2	Poland	1
Spain	2	Sweden	1

TABLE II
DISTRIBUTION OF PAPERS ACCORDING TO SUBJECT-MATTER

	U. S.	All other countries	Total
Tests and measurements.....	81	9	90
General and theoretical considerations (significance and utility of tests, technique and statistical procedure)	22	3	25
Intelligence tests ²	18	4	22
The nature of <i>g</i>	14	2	16

² In six of these the intelligence test is incidental to the main investigation.

TABLE II (Continued)

	U. S.	All other countries	Total
Tests of growth and development.....	9	..	9
Tests of character traits and of personality	7	..	7
Measurement of "higher processes".....	6	..	6
Measurement of emotion.....	3	..	3
Measurement of "interests".....	2	..	2
Applied psychology	60	12	72
Clinical psychology, psychiatry, and medical psychology	23	5	28
Educational psychology	16	1	17
Legal psychology	13	2	15
Psychology in industry.....	8	4	12
Child psychology	36	9	45
Adjustment problems	10	1	11
Development	8	..	8
Reflexes	25	8	33
Conditioned reflexes	9	5	14
Psychogalvanic reflex	7	3	10
Other reflexes and general and theoretical considerations	9	..	9
Animal psychology	29	2	31
Problems of learning.....	13	..	13
Instinct or innate behavior <i>vs.</i> learning or environmental influence	7	..	7
Conditioned reflexes	2	2	4
Systems and fields of psychology.....	16	9	25
Memory ³ (learning, association, and habit)....	20	4	24
Emotion	12	11	23
Personality	19	3	22
Types	3	1	4
Character	5	..	5
Traits	4	..	4
Aesthetics	17	5	22
Theory of aesthetic feeling, etc.....	10	3	13
Applied aesthetics	7	2	9
Music	6	2	8
Physiological psychology	16	3	19
Motor activities and skills.....	13	2	15
Eye-movements	5	1	6
Statistics	15	1	16
General discussions	3	..	3
New formulæ, etc.	12	1	13

³ This includes work on human beings only.

TABLE II (Continued)

	U. S.	All other countries	Total
Abnormal psychology exclusive of psycho-pathology	8	7	15
Hallucinations	2	2	4
Hypnosis	2	..	2
Suggestion	1	..	1
Instinct	11	3	14
Apparatus	12	1	13
Meaning	8	3	11
"Higher processes"	7	4	11
Perception	5	5	10
Sleep	10	..	10
Social psychology	6	3	9
Sensory capacities	5	3	8
Race	4	4	8
Motivation and interests	7	..	7
Psychophysics	4	3	7
Drugs	5	1	6
Religion	5	1	6
Speech	3	2	5
Philosophical subjects	2	3	5
Historical subjects	3	1	4
Dreams	1	1	2
Images	1	..	1
Eidetic images	1	..	1

TABLE III

DISTRIBUTION OF PAPERS ACCORDING TO TREATMENT OF SUBJECT

	U. S.	Ger- many	Great Brit- ain	U.S.S.R.	All others	Total for- eign	Total
Analysis	121	15	15	3	18	51	172
Theoretical, philosophical, and historical	22	5	3	..	5	13	35
Specific topics	72	9	9	3	13	34	106
Problems and methods	27	1	3	..	..	4	31
Experiment	118	6	2	10	11	29	147
Objective method	94	5	..	8	5	18	112
Traditional method	14	1	1	2	4	8	22
Introspective method	10	..	1	..	2	3	13
Real life observations	31	4	..	2	9	15	46
Tests	20	1	..	1	1	3	23
Combination of test and real life observation	23	..	1	..	..	1	24
Unclassifiable	2	..	..	..	..	..	2

TABLE IV

DISTRIBUTION OF PAPERS ACCORDING TO SYSTEMATIC POINT OF VIEW

	U. S.	Ger- many	Great Brit- ain	U.S.S.R.	All others	Total for- eign	Total
Prime interest in							
Gross behavior	136	4	1	10	5	20	156
Physiological facts ⁴	19	1	1	1	2	5	24
Behavior with interpreta- tion in terms of con- sciousness	22	1	2	1	3	7	29
Correlation of stimulus and conscious response..	20	3	1	..	3	7	27
Mental tests	43	1	1	1	3	6	49
Facts of consciousness....	37	13	8	..	20	41	78
Unclassifiable	21	3	2	..	1	6	27

TABLE V

	U. S.	Ger- many	Great Brit- ain	U.S.S.R.	All others	Total for- eign	Total
Polemical or explicit ad- vocacy of							
Behaviorism or objecti- vism	7	..	1	..	..	1	8
Mentalism	..	2	..	..	..	2	2
<i>Gestalt</i> psychology	..	1	..	..	..	..	1
Dynamic psychology	1	..	..	..	..	..	1
Motor psychology	1	..	..	..	..	..	1
Act psychology	1	..	..	..	..	..	1
Psychical dynamism	..	..	..	..	1	1	1
Eclectic positions	7	..	1	..	3	4	11

⁴ In eight of the papers in this class the interest was in the effect of some physiological condition on gross behavior. All of these were contributed by Americans. In the rest of the papers in this class, the interest was in the physiological process itself.

SPECIAL REVIEWS

D. W. PRALL. *Aesthetic Judgment*. New York: Thomas Y. Crowell Company, 1929. Pp. xvi+378.

Dr. Prall's book is a most readable, thorough and highly discriminating treatise on what Clive Bell calls *significant form*, and which Dr. Prall terms surface qualities of experience, which constitute for him the substance of beauty when viewed with discrimination.

"Discriminating perception focussed upon an object as it appears directly to sense, without ulterior interest to direct that perception inward to an understanding of the actual forces or underlying structure giving rise to this appearance, or forward to the purposes to which the object may be turned or the events its presence and movement may presage, or outward to its relations in the general structure and the moving flux—such free attentive activity may fairly be said to mark the situation in which beauty is felt" (p. 57). The aesthetic judgment is a record, a pronouncement, of the occurrence of such free, direct, spontaneous, intuitive, intrinsic, pure, nonpractical activity. The aesthetic activity begins "where mechanics and physics and biology, economics and politics and ethics, end, where most of what we know of life is perfected into habits or even cessation" (p. 3). It is always present, however, as the background and substratum of practical experience and in those off-moments when we are delivered from the pressure of practical demands; "when the pressing occupations of practical effort either tire us or leave us for a moment to our own devices, as when in the absorbing business of driving at forty or fifty miles an hour along a highway to get to a destination, the tourist on his holiday glances at the trees or the hills or the ocean, or when, in sheer exhaustion, from a desk covered with letters to answer, or accounts to disentangle or invoices or contracts to check over, we look away and instead of turning to the clock to see when we must stop to meet an appointment, or when we may stop without prejudice to our wages, we turn instead, perhaps only for a moment, to the commotion about us, listen to the vast city noises roaring outside, notice the worried look of a face bent over another desk, the odd self-confident tone of a voice giving instructions, the monotonous repetition in the cadence of the telephone operator's answers and calls, the expertness of her manipulation of

the switch-board, the rattle of the typewriters, the banging of the surface-cars, the smoky shafts of light striking through dull window-panes, the enveloping odor like hot glue that comes from the packing-house nearer the river, or even the image of the opaque river itself, swirling in oily patches under the bridge past the embankment" (pp. 31, 32).

Beauty is therefore coextensive with the whole realm of human experience and not limited to the sphere of art. Its sole demand is discriminating senses which can select "specific features of the world which thus become distinctly characterized objects in perception. Such selection and the further discrimination of details in these chosen formal objects are the sole avenues to aesthetic pleasure. And this means that without discriminating senses, and without cultivation of these senses, one remains in elementary and primitive ignorance of the world as it is actually presented to us to dwell in and to dwell upon" (p. 37).

Dr. Prall justly calls attention to the difference between the aesthetic judgment and the aesthetic experience. "Aesthetic judgment is distinguished from aesthetic experience as such by the simple fact that it follows and records such experience after the experience has been had, and in reference to what was experienced" (p. 51). Nor is aesthetic judgment, criticism. Aesthetic judgment "makes no attempt at first to say that one thing is better than another, nor to explain on what grounds it is so, nor to say just what class of things it belongs to, nor to explain it in any sense, nor yet to reproduce in an account of the thing some of its quality of beauty so that a reader of the criticism may reenact a similar aesthetic moment of more or less derivative appreciation" (p. 5). In other words, it is not dogmatic, dictatorial, nor descriptive. It is simply a report, a record, of a vital experience. Dr. Prall's book is limited to a study of the nature and the basis of this judgment, and he thus happily avoids the confusion and the muddled thinking that is found in too many books on aesthetics.

Dr. Prall has made in this book a most welcome and valuable contribution to the literature of aesthetics, written by a penetrating, clear thinker, and a fascinating stylist. He neither attacks other theories nor does he anywhere imply that his own is impregnable, a virtue but too rare in writings on this complex subject.

MAX SCHOEN.

Carnegie Institute of Technology

W. B. PILLSBURY. *The History of Psychology*. N. Y.: Norton, 1929. Pp. 326.

This very well written history places before the reader the principal "schools" and systematic positions that have appeared in psychological thinking since the time of the early Greeks until today. The treatment consists largely of dealing with certain selected men with a statement of their psychological positions (and a brief biological statement) and the development of each systematic position in the continuum of psychological thought. These statements of systematic position are excellently abstracted with all of the primary aspects given and with just enough detail to explain each system. The very early systems are dismissed with a few pages. Almost half of the book is given to a discussion of the empirical systems from Descartes to the beginning of the experimental period. The rest of the volume is concerned with post-experimental systems. This relative emphasis is so different from that in histories of psychology in the past that it cannot be called too clearly to the attention of the reader. In the past most histories have stopped just short of the experimental period and have dismissed these systems, in which most of us are so primarily interested, in a chapter or less. It is this relative emphasis of the later work and the clear and concise form of exposition which highly recommends the book to the student who attempts his first orientation to the historical background and development of psychological systems.

S. W. FERNBERGER.

University of Pennsylvania

CARL MURCHISON (Editor). *The Psychological Register*. Worcester: Clark University Press, 1929. Pp. ix+580.

The volume contains the personal and scientific data of some 1,300 psychologists from all over the world. For each is given a short *vita* and this is followed by the list of his psychological publications. Slightly more than half the volume is concerned with American psychologists. The rest of the volume contains the material for individuals in the British Empire, Austria, Belgium, China, Czechoslovakia, Denmark, Finland, France, Germany, Holland, Hungary, Italy, Japan, Norway, Poland, South America, Spain and Portugal, Sweden, Switzerland, and the Union of Socialistic Soviet Republics. The work has been excellently done and the reviewer has found only one serious omission in the name of Höffding. This work, which

was excellently conceived and remarkably well carried out, should be of great value to professional psychologists both for the purpose of obtaining data for foreign psychologists (there is no reference book outside of America which corresponds to the American Men of Science) and especially for bibliographical purposes.

SAMUEL W. FERNBERGER.

University of Pennsylvania

W. S. FOSTER and M. A. TINKER. *Experiments in Psychology*. Revised Edition. New York: Holt and Co., 1929. Pp. xv+392.

This is a thorough revision by Professor Tinker of the University of Minnesota of the former edition by Foster, which was published in 1923. Individual experiments are rewritten, some new ones added, and several dropped. The statistical treatment is revised, although the same topics are taken up.

The book is an outgrowth of the Introductory Laboratory Course given by Professor Tinker at Minnesota, and consequently is composed of experiments suitable for large classes, where considerable duplication of expensive apparatus is out of the question. It is very suitable for use anywhere, and contains enough experiments so that an instructor may choose those he wishes or modify to suit individual requirements.

The experiments dropped from Foster's original list are: Weber's Law, Taste and Smell, Filled and Empty Time, and Reflex Organic Responses—the first three as probably antiquated, and the last because it calls for too complicated apparatus. New ones added are: Two on Visual Phenomena, Span of Visual Apprehension, Individual Differences (in simple mental traits), Card Sorting, The Learning Curve, (Demonstration of) Animal Learning, Rational Learning, Eye Movements in Reading, Seashore Music Test, Affective Value of Colors, and Art Talent.

The reviewer feels that a course in Experimental Psychology should attempt to cover all possible fields of Psychology, with a few typical experiments in each given, in order to give students a well rounded sampling. The fields covered in this manual are: Sensation, 8 experiments, one cutaneous and the rest vision; Learning, 6; Experimental Aesthetics, 5; Simple Motor, 3; Pencil and Paper Tests, 2; Mentalistic, 2; Higher Processes, 1; and 5 miscellaneous, including Free and Diagnostic Association, Judgments of Intelligence and Emotion from Photographs, and Eye Movements in Reading.

(The classification is the reviewer's.) The list seems well rounded with the possible exception of the inclusion of too many of the typical structuralistic type, particularly those on sensation and imagery. Learning is very well treated, however, and the experiments in this group are well distributed over the field.

The statistical treatment is minimal, and the manual contains in the appendix numerous tables to facilitate calculation. Techniques are introduced along with experiments which demand them, rather than being grouped in a body. The reviewer thinks that a rather serious mistake was made in the effort to simplify the notation. The notation as used in Kelley's and Garrett's texts is accepted almost universally, and any divergence means that a student continuing in the field will have to unlearn and learn anew later.

RICHARD W. HUSBAND.

University of Wisconsin

R. R. GATES. *Heredity in Man*. (Rev. Ed.) New York: Macmillan, 1929. Pp. xiii+385.

Mr. Gates is without possibility of doubt a most indefatigable *Sammelreferent*. In this single volume of modest size he appears to have collected most of the standard data bearing on his subject, as well as not a little bearing on general genetics. That some of the material most important to the psychologist is conspicuous by its absence, and that much of that which finds place is rather unevenly digested, is perhaps not surprising. The style suffers somewhat by comparison with works both higher and lower in the scale of popularization—with books like Crew's and Morgan's, as well as with ones like Whitney's *Basis of Breeding*. The use of the masculine personal pronoun in referring to Downey's work on handedness and the parenthetical note placing Long Island in Connecticut, while trifling in themselves, are perhaps diagnostic of the difficulty—one's viewpoint must be encyclopedic and one's capacity for inspiration unlimited to see a field like this fruitfully, and Mr. Gates, though a fine thoroughgoing pedestrian, will never be an aviator.

There are two or three things the psychologist looks for—if he looks for anything at all—when the publisher shoves another genetics book at him. What, in the first place, has been done that might throw any light on the untangling of multiple-factor situations? Nor is it necessary to hold the discussion down to intelligence as a material, or even stature; it happens that at the present moment one

of the most significant studies bearing on the inheritance of intelligence is that by Gowan on milk and butter fat production. Second, what is the latest story on linkage, including the sub-problems of correlated traits and racial differences? Third, what of the perennial battle between Freud and Mendel? It must be admitted that the picking in the volume under consideration is pretty poor; most of the book is taken up with more or less rare morphological recessives, mono- or digenic. There is no mention of the masterly analysis that made Fisher's earlier papers the high water mark in the biometric analysis of Mendelian heredity; in fact, there is very little of the biometric approach whatever. The provocative paper by Wilson on assortative mating is missing (assortative mating being represented only by passing allusions anyway) and the significant analyses by Burks, Merriman, etc., of heredity in mental characters are nowhere to be seen. We are concerned mostly with curiosities reminiscent of Havelock Ellis—polydactyly, baldness, osteopsathyrosis, polymastia, anodontia, choroideremia, albinism, etc., etc. The author is pretty up-to-date and common-sense about all this; for instance, we note for the first time a critical point of view on the inheritance of eye color, wherein it is remarked that actual as distinct from arm-chair eyes are being found to have all variations of color; it is, of course, pleasant to have the obvious officially recognized in this way, but it cannot be said to get us very far in the study of fundamental problems.

On the multiple-factor situation we have, besides numerous speculations as to the number of factors which may be involved in sundry peculiarities, a rather nicely written chapter on the inheritance of stature, commendably stressing the technique of correlating subdivisions of a general trait with each other as a test of general growth factors versus single factors for each component; this may be considered in connection with the remarks on Spearman's g (Kelley's addition unnoticed), which, however, appears in another part of the book.

The linkage matter is mentioned now and again, as in the section on eye and hair color; passing references are made to it in the course of the discussion on race crossing. The whole affair seems rather lame (with the single exception of sex linkage); but to be sure, the state of knowledge is itself not as yet very robust. No discussion of the possibilities of correlational analysis is to be found here, which seems like muffing a good chance.

In Freudian fields the author possesses some sophistication, but not much information. His section on asthma, under allergic diseases, is innocent of any hint that this affliction is often familial on a hysterical basis. In discussing dipsomania he submits without adverse criticism the hypothesis that while males are affected, females may be carriers. In fact, in all matters with which the psychologist deals directly his stand is relatively uninformed; neither the section on handedness nor that on intelligence bears any reference to the work of Travis on action currents; under musical ability he accepts Seashore's results without mentioning that they were obtained with an unreliable instrument; his analyses of mental disturbance are classificatory rather than functional; he thinks of feeble-mindedness as a condition of arrested development, and suggests a positive correlation between intelligence and insanity; in spite of admitting that feeble-mindedness is well known to be continuous, he is more than slightly inclined to think of it as monogenic; he feels that handwriting is an excellent index of character. There is, however, a refreshing return to the critical outlook in the kindly remark that although Davenport's analyses of temperaments are obviously inadequate they show courage in attempting to explore a difficult field.

There is certainly a place for a volume which will gather up the *significant* data on human heredity, digest them thoroughly, and present them with firm, bold strokes on the canvas of evolution; but it should be the work of a creative genius, and however admirable Mr. Gates' intentions and industry, there is little of the creator about him. For all that, the work may easily fill a need quite satisfactorily, given a specialized audience of modest capacities and ambitions.

RAYMOND R. WILLOUGHBY.

Clark University

DESCOEUDRES, ALICE. *The Education of Mentally Defective Children*. Psychological Observations and Practical Suggestions. (Translated from the second French edition by Ernest F. Row.) Boston: D. C. Heath & Co. (no date). Pp. 313.

This is an English translation from the French book by Descoeudres, who is an authority on the training of mentally defective children, and who has had wide experience in this work in Switzerland and Belgium. In Belgium she worked with Dr. Decroly and his influence is apparent throughout the book. The now well-known Decroly method of education was begun as a method of

instruction for mentally defective children and later on extended to the education of normal and superior children. The author of this book has borrowed much of the Decroly procedure; she has rejected parts of it, and modified others. In particular she has made much use of the numerous and ingenious Decroly games for teaching purposes, elaborating some and adding others of her own. Separate chapters of the book are devoted to such topics as Training the Senses, Physical Training, Hand Work, Drawing, Speech, Reading, Arithmetic, Spelling and so on. These chapters are full of practical suggestions and concrete illustrations of the responses of mentally defective children by one who has long been teaching them. Every teacher of special classes in this country would certainly come across valuable suggestions for class-room work in these pages, regardless of whether she may or may not agree with every method advocated by the author.

The author discusses briefly the history of special classes and the general examination before the transfer of a child to a special class. This examination should be pedagogical, psychological and medical. The pedagogical should consist of measures of educational attainment, the psychological of the Binet-Simon tests, and the medical of a thorough physical examination. Special classes in ordinary schools are considered a make-shift until special schools can be organized, because in the special school more homogeneous classes may be arranged. The special class has justified itself from a social standpoint because of the fact that one follow-up study of 2,258 special class pupils showed that 58 per cent of the boys and 60 per cent of the girls were found capable of earning their entire living, and 29 per cent of the boys and 28 per cent of the girls were capable of partially earning their living.

The author is to be congratulated on her interesting book and the translator on his admirable translation into English.

R. PINTNER

Teachers College, Columbia University

BOOKS RECEIVED

ANNALIES ARGELANDER, *Das Farbenhören und der synästhetische Faktor der Wahrnehmung*. Jena: Fischer, 1927. Pp. 172.

KURT LEWIN, *Die Entwicklung der experimentellen Willenspsychologie und die Psychotherapie*. Leipzig: Hirzel, 1929. Pp. 28.

DAVID A. OREBAUGH, *Crime, Degeneracy and Immigration. Their Interrelations and Interactions*. Boston: Badger, 1929. Pp. xvi+272.

A Study of Problem Boys and Their Brothers by the Sub-Commission on Causes and Effects of Crime. Albany: Crime Commission of New York State, 1929. Pp. 408.

RÉNE GUYON, *Réflexions sur la tolérance*. Paris: Alcan, 1930. Pp. 113.

K. S. LASHLEY, *Brain Mechanisms and Intelligence. A Quantitative Study of Injuries to the Brain*. Chicago: Univ. of Chicago Press, 1929. Pp. xiv+186.

M. V. O'SHEA, *Newer Ways with Children*. N. Y.: Greenberg, 1929. Pp. ix+419.

HILDEGARD HETZER, *Kindheit und Armut*. Psychologische Methoden in Armutsforschung und Armutsbekämpfung. Leipzig: Hirzel, 1929. Pp. xii+314.

FRITZ KÜNKEL, *Vitale Dialektik. Theoretische Grundlagen der individualpsychologischen Charakterkunde*. Leipzig: Hirzel, 1929. Pp. viii+134.

HAROLD H. G. HOLCK, *Diet and Efficiency. A Five-Year Controlled Experiment on Man*. Chicago: Univ. of Chicago Press, 1929. Pp. ix+72.

ROBERT M. YERKES and ADA W. YERKES, *The Great Apes. A Study of Anthropoid Life*. New Haven: Yale Univ. Press, 1929. Pp. xix+652.

HARRY CHAPLIN, *Gianfrancesco Pico della Mirandola On the Imagination*. The Latin Text with an Introduction, an English Translation and Notes. New Haven: Yale Univ. Press, 1930. Pp. ix+102.

A. J. I. KRAUS, *Sick Society*. Chicago: Univ. of Chicago Press, 1929. Pp. x+206.

FRANK C. SHARP, *Ethics*. N. Y.: Century, 1928. Pp. vi+566.

ROBERT F. WHEELER, *The Psychology of the Spirit*. Boston: Badger, 1929. Pp. 250.

E. R. HAMILTON, *The Art of Interrogation*. Studies in the Principles of Mental Tests and Examinations. (Introduction by C. Spearman.) N. Y.: Harcourt, Brace, 1929. Pp. xii+174.

HENRI PIÉRON, *Le développement mental et l'intelligence*. Paris: Alcan, 1929. Pp. xii+95.

STANLEY P. DAVIES, *Social Control of the Mentally Deficient*. N. Y.: Crowell, 1930. Pp. xix+389.

BENNO KERN, *Wirkungsformen der Uebung. Beitrag zur Psychologie der Arbeit*. Münster: Helios-Verlag, 1930. Pp. ix+508 (with 3 plates).

PAUL H. FURFEY, *The Growing Boy*. N. Y.: Macmillan, 1930. Pp. viii+192.

Survey of College Entrance Credits and College Courses in Music. N. Y.: Nat. Bur. Advancement of Music, 1930. Pp. vii+209.

MAURICE GARCON and JEAN VINCHON, *The Devil. An Historical Critical and Medical Survey*. (Trans. by S. H. Guest.) N. Y.: Dutton, 1930. Pp. 288.

ALFRED ADLER, *Problems of Neurosis*. (Preface by F. G. Crookshank.) N. Y.: Cosmopolitan Book Corp., 1930. Pp. v+244.

DONALD A. LAIRD and CHARLES G. MULLER, *Sleep. Why We Need It and How to Get It*. N. Y.: John Day, 1930. Pp. x+214.

CARL N. REXROAD, *General Psychology*. N. Y.: Macmillan, 1929. Pp. xv+392.

WALTER S. MONROE, JAMES C. DEVOSS and GEORGE W. REAGAN, *Educational Psychology*. N. Y.: Doubleday, Doran, 1930. Pp. xiii+607.

LOUIS GRUDIN, *A Primer of Aesthetics. Logical Approaches to a Philosophy of Art*. N. Y.: Civici, Friede, 1930. Pp. xvi+247.

W. TROTTER, *Instincts of the Herd in Peace and War*. N. Y.: Macmillan, 1930. Pp. 264.

R. MONEY-KYRLE, *The Meaning of Sacrifice*. London: Hogarth, 1930. Pp. 273.

FRANK K. NOTCH, *King Mob. A Study of the Present Day Mind*. N. Y.: Harcourt, Brace, 1930. Pp. 226.

NOTES AND NEWS

DR. KIMBALL YOUNG, formerly associate professor of sociology in the University of Wisconsin, has been made a full professor in that university.

DR. W. V. BINGHAM, Director of the Personnel Research Federation, has been appointed lecturer in psychology at the Stevens Institute of Technology.

THE First International Congress of Religious Psychology will be held in Vienna from May 26 to May 31, 1931. For further information about programs, etc., address Professor Karl Beth, Vienna, VII. Litterhofergasse 8, Austria, enclosing 50 cents in stamps.

DR. OSCAR EDWARD HERTZBERG, formerly professor of educational psychology in the Colorado State Teachers College at Greeley, has accepted a position as head of the new department of educational psychology at the New York State Teachers College at Buffalo.

DR. CHARLES SCOTT BERRY, professor of educational psychology at the University of Michigan and consultant in special education for the Detroit public schools, has been appointed to the staff of the State Department of Education of Ohio in the newly created office of consultant in the education of the mentally handicapped.

DR. CHARLES H. JUDD of the University of Chicago has been appointed to the Charles F. Grey Distinguished Service Professorship at that university.

DR. MARTIN L. REYMERT, formerly head of the Department of Psychology at Wittenberg College, has been appointed Director of Child Research at the laboratory now being established at Mooseheart, Illinois.

FOR A CHANGE IN THE DATE OF THE ANNUAL MEETINGS OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The question of holding the annual meetings of the American Psychological Association in September instead of during the Christmas vacation will doubtlessly be raised at the next annual business meeting of the Association. The desirability of this change in date became apparent at the Yale meeting of the International Congress. It is probable that had there been an annual business meeting of the Association last year and had the change then been proposed it would, with the memories of the Yale meeting fresh in mind, have been favorably received. Since, however, tradition is strong and the memory of the seasonal advantages enjoyed by the Yale meetings is apt to be fleeting, the proposal may now be opposed. It is with the effect of tradition and inertia in mind that I venture to recall the Yale Congress and to list some of the advantages of September over December.

(1) *Travel.* Traveling conditions are better in September than in December. Not only will those traveling by automobile be benefited by the change in date, but those traveling by train will also find conditions more comfortable and pleasant. Cold and drafty, or hot and stuffy, trains are not the rule in September as they are in December.

(2) *Accommodations.* If the meetings are held in September before the opening of the fall term, most of the universities at which we may meet will open their dormitories to us. This will make not only for a material reduction in expense but, as recent experience has shown, for successful meetings. Where the membership has been housed under one roof and has not been diluted with non-psychological guests, there have been held our most pleasant and profitable gatherings.

(3) *Weather conditions.* Weather conditions as a rule are much more favorable in September than in December. Not only will the formal meetings be held under more comfortable circumstances, since heating and ventilation are not problems in September, but the informal meetings, which are perhaps the most delightful at the annual conventions, may be held out-of-doors instead of in overheated and smoke-laden rooms. Between programs the members will also be able to enjoy walks and tours of inspection around the campus of the host-university.

(4) *Health.* If the annual meetings are held in September fewer members will return home ill, not only because of the more favorable

traveling, housing, and meeting conditions mentioned above, but also because pandemics—colds, influenza, and grippe—are not so prevalent then.

(5) *Section I of the A.A.A.S.* If the meetings of the American Psychological Association are held at times different from those of the A.A.A.S., Section I will be permitted to develop freely and naturally. Under present conditions—alternately rival and joint meetings of the two Associations—Section I plays a minor rôle in the development of psychology. When separate meetings are held Section I has a small attendance and its programs are not up to the standard of the other Sections of the A.A.A.S.; and when joint meetings are held, Section I does not function. The present situation is not conducive to the development of Section I.

Pressure for place upon the annual programs of the American Psychological Association is sufficiently great—as the annual reports of the past few years attest—so that two national meetings of psychologists could profitably be held every year.

(6) *Family.* If there is one time during the year more than any other when a member wishes to be with his family that time is during the Christmas holidays. Yet the annual meetings of the Psychological Association now afford the only opportunity for the renewal of professional acquaintances. The September date would resolve this unfortunate conflict.

(7) *Graduate students.* A minor point, but one which is significant if the "Graduate Students" section is to be what its name implies and not a section for instructors, is that the September date of meeting permits a student who is not yet an instructor to report upon a completed piece of work. In December the studies of graduate students are usually in the making; those having completed studies are almost invariably instructors with the doctorate.

One argument against the September date of meetings—whose cogency I do not admit—is that the summer vacations, particularly of those journeying to Europe, will be cut short. Those spending their vacations in this country will, I believe, find the September meeting a continuation of their holiday and a fitting close to it. The benefits and suggestions obtained from the meetings could be put into immediate practice, when the fall work is initiated and undertaken. Those spending their vacations in Europe might have to forego the meetings for that year, but they would have the meetings of Section I at Christmas time before them. In any case the numbers affected in any given year would not be great.

Columbia University

KARL M. DALLENBACH

THE PSYCHOLOGICAL BULLETIN

THE FIELD OF PERSONALITY

BY GORDON W. ALLPORT AND PHILIP E. VERNON

Harvard University

It is no longer possible, as it was in 1921 (10), to present in a single review for the BULLETIN a fairly complete survey of the psychological studies in the field of personality. Interest in the subject is largely a post-war phenomenon, but though recent it has already reached astonishing proportions. Summaries are not lacking, but none is in itself complete, and few attempt a critical classification and interpretation. The present review is intended primarily for the orientation of the general psychological reader who wishes to acquaint himself with the methods and problems of contemporary research in personality. It may also help the investigator of some specific topic, who will find here a point of departure for the study of his particular problem. The references employed, though far from exhaustive, are representative, and probably include most of the important contributions of the past decade, with special emphasis upon recent years. The material will be presented under the following headings:

- I. General Treatises
- II. Definitions
- III. Types
- IV. Genetic Studies
- V. Experimental Methods
- VI. Consistency and Trait

There are several intentional omissions from this list. Popular writings on how to develop "personality," personality in business, and the like, need not concern us. Owing to their very lack of academic preconceptions, however, such works may occasionally be original and stimulating; Pitkin's recent book (220) is a good

example. The literature on character education is neglected here, since it does not, in the main, advance the scientific study of personality. The treasure-troves of fiction and biography are also unexploited.

The field of psychopathology receives only incidental treatment; multiple personality, for example, being wholly omitted. Only a representative fraction of the work on constitutional make-up or endocrinology could be included. The genetic field and the sociological approach are merely sampled in order to disclose their chief methods and theories. Literature dealing with particular traits, with genius or with intelligence, is only included when it bears on the general theory of personality. Such topics as leadership, emotionality, and humor deserve independent surveys; so far originality (71), social intelligence (268a), radicalism-conservatism (293), and extroversion-introversion (112) seem to be the only traits that are separately reviewed. Since 1925 the field of testing has been thoroughly surveyed annually by May and Hartshorne (193, 194, 196), so that, in general, only those investigations in measurement will be mentioned whose theoretical or methodological implications are of special importance.

Russian work receives little treatment here; the most accessible general statements of the Russian point of view are contained in the German volume by Bechterev (31) and in the English articles by Schniermann (243) and Kornilov (161). An attempt is made, however, to give to German writers the leading position which they deserve in this field.

Among previous bibliographies on personality Roback's (236) is the most inclusive; but since it was published in 1927, the majority of our titles fall outside it. Allport's early review (10) gives 71 references, and in 1927 (13) he attempted to clarify some of the problems of definition. Most of the 46 titles in the latter article are omitted. Manson's bibliography (185) up to 1926 includes mainly the practical and applied studies in personality. The Proceedings of the Second Colloquium (230) gives about 300 titles. Several other bibliographies which refer to more specialized topics will be cited later in the text.

I. GENERAL TREATISES

Under this heading are included books which, by their title or their contents, claim to consider the field of personality in a comprehensive manner. When one surveys the volumes, however, it is all

too evident that *Titel machen Bücher*. Bagby (25) gives too spacious a title to his little essay on tension and its reduction. Gordon (108) surveys the psychoanalytic schools and adds a theory which is a mixture of Gestalt and Emergence. Elliot's work (83) is a popular psychiatric psychology, and Valentine's (290) is an elementary text. It seems fair to omit many other works which, like Grove's (110) and Brown's (54) use an arresting title to refer to general problems that have little relation to the unique issues of personality. In a later book (55) Brown moves even further toward a philosophical and religious viewpoint.

The collection of studies, *Problems of Personality* (229), contains few essays of direct relevance. But Campbell's "On Recent Contributions to the Study of Personality" covers the developments in the psychiatric field; Jung's "Psychological Types" is a short presentation of his book of the same name, and Roback's "Character and Inhibition" is a curtailed version of his *Psychology of Character* (235). The latter book ranks foremost among the more solid treatises, and in scope and documentation exceeds anything so far published. It is the standard reference for the historical aspects of the subject, and contains a detailed theory of character; it has already been reviewed at length in the BULLETIN (14). A comparable work, but less explicit and entirely undocumented, is that of Utitz (288), which he has recently supplemented by a characterological study of present day culture (289). Ziehen (326) regards characterology as a "confluent" science and therefore presents a large and interesting variety of material in his volume. From the French we have the work of Achille-Delmas and Boll (2), in which personality is regarded as an *ensemble* of innate (intellectual and affective-conative) and acquired functions. Their whole theory is ordered under the concepts of psychostatics and psychodynamics, and gives as formal and Comtean a doctrine as that of past generations of French writers, *e.g.*, Malapert, Azam, Ribéry and Paulhan.

In Germany, where greater interest is shown in problems of personality, there are three figures whose influence has been so marked as to create definite schools of thought, Klages, W. Stern and Spranger. Certain tenets of these schools are reserved for subsequent discussion, but their general work is cited here.

Klages, known in this country for his graphology, has extended his work on expressive movement into a psychometaphysical system which is both imposing and difficult (151, 152, 153). He is not an academic psychologist, and ridicules what he calls "school psy-

chology" for its neglect of its most valuable data, namely, speech and graphic movement. Klages draws heavily upon Nietzsche and upon his own literary imagination for his unique categories for the classification of men, and for his treatment of will. Prinzhorn (228) follows Klages rather closely and applies his basic ideas to psychiatry.

Stern (264) has built upon more authentic psychological foundations a personalistic Aristotelian idealism. His work thus has both a philosophical and psychological aspect. The former he calls *Personalismus* and the latter *Personalistik* (267). His method is dialectical to an extreme degree, and in a formal fashion provides perhaps more adequate categories for the intricate manifestations of personality, especially of its goal-seeking aspects, than any other system. On the strictly psychological side he has shown the consequences of his views for tests of personality (266) and for applications in the law courts (268). Stern's chief point of resemblance to Klages consists in his untiring emphasis upon the vital unity of personality. Personalistic psychology has been briefly reviewed by Calkins (68) in the BULLETIN.

Spranger (261) agrees with the two preceding schools in regarding the methods of natural science as wholly inadequate to the complete study of personality. His chief contributions have been the notions that what a man wants in life, *i.e.*, his personal scale of values, distinguishes and characterizes his personality; that there are a limited number of these basic scales of values; that the cultural approach is necessary for their understanding; and that *knowledge* about personality is not the same as *understanding* of it in any single concrete case. Accounts of this school may be found in articles by E. Stern (263), Klüver (155, 159), G. Allport (11, 17, 18) and Wenzl (307).

Tumlirz, like Utitz, surveys the problems of characterology in general (287), while the books of Wahle (297), and Ewald (85) are concerned chiefly with their own theories of the innate temperamental foundations of personality. Hoffman (132, 133) gives a review of the work of Spranger, Müller-Freienfels, Weininger, Jung, Kretschmer, Klages, Apfelbach, Ewald, Kronfeld, Häberlin and others. Hoffman himself favors a biological-hereditary theory, and believes it possible to isolate the characterial elements in heredity. These and similar books on the constitutional bases of personality are reviewed by Bogen (42).

Künkel (166, 167), like Stern, uses a dialectical method. His central concept is that of "limiting factors." We cannot study an

individual's personality directly, but we can ascertain with considerable exactness the limits within which his behavior varies. It is not possible, for example, to establish patience as a trait, but we may find the limits to a man's patience. In actual practice, the studies on character education by Hartshorne and May (120, 121, 122) follow this principle and disclose its laboriousness.

II. DEFINITIONS

Since it is rare indeed to find one author who accepts the definitions of another, there seem to be virtually as many definitions of personality, character and temperament as there are writers on these subjects. Although at first glance the problem resembles chaos, yet the principal disagreements concern the point in a definition where the emphasis falls. There are fortunately a limited number of points of stress, so that it seems possible to distinguish certain standard usages or general types of definition. But first it is only fair to record that many psychologists declare that personality is indefinable. Burnham (63) writes, "What personality is, everybody knows; but nobody can tell." The same writer gives an interesting collection of definitions, and the general problem of definition is also considered by Gilliland (104). The best single collection of definitions is in the *Proceedings of the Second Colloquium* (230).

A. *Personality*. Standard etymological studies of *Persona* and its derivatives by Rheinfelder (233) and Trendelenburg (284) show that at least since the fourth century there has been a double, and in a sense antithetical meaning of the word. The mask, the assumed appearance, the visible outward manner; and the innermost moral life, the true self, the substance or *hypostasis*; both conceptions are equally entitled to the term "personality." As might be expected these two historical connotations play a part in the present confusion, although they are by no means solely responsible for it.

Superficially it would seem that the first of these meanings corresponds to the common psychological use of the term, and the second to the philosophical. (The concept of personality is fully as lively in contemporary philosophical thought as in psychological theory). Indeed C. C. J. Webb (302) considers this general usage of personality as the ego or self as standard for theological and philosophical writers since Boethius at the beginning of the sixth century. *Persona est naturae rationalis individua substantia*. The separation between the inner and the outer aspects of the self is roughly observed in German, as Ehrlich (82) has shown, in the two words

Personalität and *Persönlichkeit*. By no means all German philosophers, however, abstain from the use of *Persönlichkeit*. Stern (267) distinguishes the abstract philosophical enquiry (*Personalismus*) from the more concrete psychological enquiry (*Personalistik*). In English unfortunately both the will and the words for this serviceable distinction are lacking; but Lloyd Morgan (203) wishes to contrast the comprehensive, socially conditioned *personality* with the inner, unique *individuality*, a view which is also discussed by Ritter (234).

Now psychological definitions are not interested in the "individual substance of rational nature," but concern themselves rather with specific and concrete manifestations of this abstract principle. Yet such definitions range in their point of emphasis from the most superficial outward aspects of behavior (the mask) to the inner psychological states that can be inferred from these expressions, *e.g.*, sentiments, complexes, traits and integration. In general these opposed standpoints correspond to the behavioristic and the psychoanalytic approaches, respectively. An appropriate example of the former, outer, view, is May's (190, 191). He writes, "an individual's personality is not defined by the responses he makes to others, but rather by the responses others make to him as stimulus. In brief, personality is an individual's social stimulus value." What man really is is not here in question; the personality is its appearance as determined by its effect on others. The more effect (of whatever kind) the "more personality."

On the other hand Krudewig (165), who has made a thorough survey of the rôle of the concept of personality in modern theoretical psychology in Germany, finds that the outer man, the mask, is not an adequate expression of the psychological usage. And Brown (54) writes, "although the word itself, *persona*, suggests drama, even melodrama, pretence, hypocrisy and the like, nevertheless when we come to consider the ordinary use of language we find that people who are regarded as having personality are just the opposite of this. They are not the people who play a part." McDougall (179, 180) stresses the importance of regarding personality as a solidly constructed system of sentiments and not as the mere mask of a poseur, while MacCurdy (176) offers the following definition: "Personality is an integration of patterns (interests) which gives a peculiar individual trend to the behavior of the organism." Such definitions frankly attribute a psychological reality to personality; it *is* something and *does* something. While they approach the philosophical

standpoint, they are not so abstract. This same point of view is represented by J. Adams (3), "Personality is the ego looked at in a more concrete way. It has more content than the bare ego."

Definitions given by psychologists may therefore be placed in a continuous series from the most outer (biosocial) to the most inner (biophysical) conceptions. But it is serviceable likewise to order them under a classification which brings forward more than this one point of emphasis.

In the review of 1921 (10) it was found that many definitions were concerned only with the enumeration of the components of personality. These were called "rag-bag" definitions at the time, though "omnibus" is perhaps a more suitable term. To the (a) *omnibus* type of definition it is possible to add the following: (b) *integrative*, (c) *hierarchical*, (d) *adjustive*, and (e) *social*.

(a) *Omnibus definitions* generally contain the term "sum-total," and are too frequently encountered to need much illustration. A typical brief one is that of Valentine (290), "Personality is the sum-total of one's habit dispositions." Sometimes a definition of this type is less explicit and may be accompanied by a gesture of despair as is the case with Menninger (198): "Of course personality is used to describe almost everything from the attributes of the soul to those of a new talcum powder. As we shall use it, it means the individual as a whole, his height and weight and loves and hates and blood-pressure and reflexes; his smiles and hopes and bowed legs and enlarged tonsils. It means all that anyone is and all that he is trying to become."

Many recent authors of omnibus definitions seem to sense their inadequacy, and add a provision, however dim, for unity or integration. Baar (24) writes, "Personality is the sum-total of an individual's tendencies to reaction both native and acquired. Each tendency is a trait and the unity of a personality is due to the integrating function of the nervous system." Some prefer to the term "composite" (Brandenburg, 46), "aggregate" (Amsden, 20), or "ensemble" (Achille-Delmas and Boll, 2).

(b) *Integrative definitions* stress the organization within the personality. Warren's (298) is often quoted—"the entire organization of a human being at any stage of his development," including, "intellect, temperament, skill and morality." Typical, also, is Fisher's (88), "The expression of the integration of the sum-total of the individual's mental aspects." He adds that few, if any, of us ever reach the level of complete mental integration. The problem of

the degrees of integration is further considered in Section IV of this review, and in the review of 1927 (13). According to Thomas (277) personality is dependent on the organization and relative dominance of four desires, namely: for new experiences, for intimate relations and contacts, for recognition and for security. Müller-Freienfels (204) emphasizes the progressive integration of the experience of the person. Smuts (257) considers personality as the latest and most developed Whole that has appeared in evolution, and stresses its unique, but often far from complete, integration.

(c) *Hierarchical definitions* are distinguishable by their notions of levels of organizations. The prototype for conceptions of this sort is to be found in James' classic treatment of the four levels of the Self. Almost the same idea is developed by Blondel (40) and by Martin (188). Bridges (49, 50) presents a thoroughgoing hierarchical theory, finding a similar type of organization and development in the cognitive, affective and conative dimensions of personality. Heider (127) draws his four levels of personality from the different *Schichten* of consciousness. McDougall's theory of personality, like Shand's, is clearly hierarchical (179, 180, 181); dominating and harmonizing all the lower integrations is the "self-regarding sentiment." Though Prince (226) also emphasizes the disunities in personality, yet his view is similar to McDougall's, and Child's (69) biological theory is explicitly hierarchical. Watson's definition of personality is not hierarchical, but his treatment of the conditioned reflex and habit systems definitely is (301).

(d) Emphasis on *adjustment* is found in biological and behavioristic definitions, either combined with an omnibus conception as in Watson's (301) "sum-total of habit systems," or with an integrative conception in Leary's (168) "whole-organism-in-action." Following Watson's earlier lead, Rexroad (232) defines personality, "as the balance between socially approved and disapproved traits." While this obviously refers to adjustment, it ushers in, at the same time, the highly confusing and totally irrelevant criterion of social approval, which belongs, if anywhere, to the concept of character (*vide infra*). Another definition which emphasizes habits or manners of adjustment is Bowden's (45).

(e) *Social definitions* are likely to come from sociologists and to refer to the status of an individual in society. Eubank (84) wishes to consider personality as the "ultimate granule of the human group," "the true societary unit," and Marcuse (186) writes, "Personality is the convergence of all essential cultural tendencies in one

mind. The more culture one has, the harder it is to be a single personality." May's view of personality as nothing but the social aspects of the individual has already been cited; related to it, but not so extreme in implication is F. H. Allport's (9) treatment of personality as result and cause of social behavior.

Another variety of social definition stresses the *distinctiveness* of the person. Thus Ritter (234) writes, "An individual's personality consists in those attributes which differentiate it from other individuals, especially of its own kind"; and Yoakum (319) calls it, "that combination of behavior forms in the individual . . . which distinguish that individual from others of a group." "Idiocyncrasy" is suggested by Gillespie (103) as a substitute for the hackneyed and ambiguous term personality; it includes individual differences and their hierarchical organization at various levels. Another definition emphasizing the integrative and distinctive is Schoen's (244), "Personality is the organized system, the functioning whole or unity, of habits, dispositions and sentiments that mark off any one member of a group as being different from any other member of the same group. If all the members of any one social group acted alike, thought alike and felt alike, personality would not exist." Similar is Wheeler's statement (310), "It is that particular pattern or balance of organized reactions which sets one individual off from another." Young's definition (325) seems to stress both integration and social status, "the sum-total of images, ideas, attitudes and habits of the individual organized in terms of his social participation."

Closely related in thought is Woodworth's "adverbial" conception (317), "Personality refers not to any particular sort of activity, such as talking, remembering, thinking or loving, but an individual can reveal his personality in the way he does any of these things." This notion is very suggestive in that it puts the emphasis squarely upon the problem of *style*, a concept almost completely neglected by psychologists. The manner of acting is considered to be the essential feature of personality likewise by Schlink (242) who distinguishes the easily altered, progressively changing *personality* from the more stable, resistant, normally unaltered *Ego*. This distinction obviously marks off the psychological problem from the more philosophical.

B. *Character*. The term "character," as originally employed by Theophrastus, clearly possessed much of the same adverbial significance as Woodworth ascribes to "personality"; and there is no historical reason why it should not be used interchangeably with personality. Indeed every meaning that has been assigned to one of

these words has, probably by some writer, been assigned to the other. Thus it might easily be argued that they are in every respect equivalent.

In modern psychological usage, however, there can be detected at least two main lines of divergence, both of which give meanings to "character" that are rarely associated with "personality." The first of these tendencies is to attribute an ethical or moral significance to the term, to consider character as personality evaluated according to some social or ethical norm. J. B. Watson earlier made this proposal, but failed to follow its consequences, which would call for the rigid exclusion of all normative considerations from the sphere of personality proper. His treatment of personality has been fraught with evaluation (*cf.* "reaction assets and liabilities"), a fact which means that, by his own distinction, he has written more of character than of personality. A clear statement of the distinction is given by J. Adams (3), "Character is more of an estimate of the individual. It should indicate moral evaluation." Admittedly there is a very strong colloquial tendency for personality to be evaluated as "force" or "charm" and the like (a tendency which is taken into account in May's *social stimulus value* theory). And the contemporary movement for character education continually faces the problem of standards. But psychological research in personality proper should never be troubled by these confusions. Just as this trend seems to be enjoying a desirable growth, it is annoying to find the exact opposite usage by Schoen (244), "Individuality produces self, self gives rise to character, and character is evaluated as personality."

The other main tendency in the use of the term "character" is to apply it to some special phase of personality, usually the conative. Filter (87), McDougall (181), Taylor (274), and Roback are writers who illustrate this standpoint. Taylor calls it "the degree of ethically effective organization of all the forces of the individual." Others consider character as a kind of stable background to personality. Clark (70) believes that an individual has as many different personalities as there are different social groups with which he comes in contact; character is then identified with the degree of organization and integration of these personalities. Similarly Thomson (279) writes, "Character furnishes the stabilizing quality of personality." Such concepts possibly derive from Dewey's theory of character as the degree of "interpenetration of habits" (77).

Roback (235) defines character as "an enduring psychophysical disposition to inhibit impulses in accordance with a regulative prin-

ciple." Here the "regulative principle" is non-psychological, but he still considers character as containing clearly marked psychological processes, and therefore as a natural datum for psychology. The view of the present writers is that, in so far as Roback and others who have a similar approach to character are dealing with drive, inhibition, perseveration, integration, or with other genuinely psychological phenomena, these can be adequately expressed as manifestations of *personality*. The word *character* might then be regarded as a purely evaluative concept and deliberately abandoned as a psychological concept. Nor will any good come from promiscuously using character as equivalent to personality.

C. *Temperament*. There is less ambiguity in the concept of temperament, though it is occasionally applied by some writers to virtually the whole field of personality. Thus Burt (65) considers temperament as equivalent to the affective-conative aspects of the total individual, whether inborn or acquired, general or specific. Bartlett (29) defines it as a group of predisposing tendencies, characterizing the individual, independent of training, arising early in life and remarkably persistent from generation to generation. He thinks that class distinctions in society may ultimately rest upon this inherited and stable foundation. Both individual differences in instincts, endocrine factors, autonomic tensions, and psychopathological tendencies are included by Bridges (48) in an eclectic concept, while Porteus and Babcock (224) also interpret temperament in a broad sense.

The great majority of writers seem to confine temperament to innate factors but place their emphasis upon one of three functions: (a) the emotional, (b) the physiological, or (c) the kinetic.

(a) A consistent treatment, stressing the first of these, is given by F. H. Allport (9), who considers temperament to be the characteristic phenomena of an individual's emotional life, including his susceptibility to emotional stimuli, the customary strength of his emotional responses, the quality of his prevailing mood and peculiarities of fluctuation and intensity in mood. Presumably, at least the foundations of such qualities are considered as innate.

(b) That temperament is closely connected with vital organs and functions is evident in a great deal of research and theory. Rayner (231), after giving a useful historical survey, considers temperament almost wholly from the physiological standpoint. Piéron (219) would seem to find its basis in the thalamus and palaeo-encephalon. McDougall (179, 182) separates off from what most writers would call temperament both *disposition* (inborn, conative-

affective tendencies) and *temper* (persistency, urgency, etc., of these "drives"). *Temperament* is then confined to the more strictly vegetative factors, "the influence, direct or indirect, of bodily metabolism (more especially of the endocrine secretions) upon the psychophysical processes of the nervous system." From this point of view Kempf's well-known work (149) must be said to deal with temperament rather than with personality. The same applies to Berman (34), whose speculative thesis has been revised and expanded (35). More moderate but still speculative glandular theories are presented by Cobb (72). Berman as well as Kempf receives the support of Williams (313) who also identifies Freud's unconscious wishes with Kempf's autonomic tensions.

Perhaps the best survey of the endocrinological field is given by Lipschütz (174). Galloway (96) has reviewed and criticized Berman, finding his doctrine a resuscitation of humors, "a metabolic phrenology." There is, he believes, no evidence that the glands completely dominate the central nervous system, nor that a chemical theory is more valid than a cerebral. A parallelistic standpoint is defended by Lewis (173), who considers that the chemical, the physical and the psychological are equally important aspects, and that all three approaches should be combined. Peters (218) attempts to show that temperament and intelligence are distinct but interactive factors.

(c) The kinetic basis of temperament has been stressed by Myerson (206) and Downey (79). More recently Bloor (41) surveys historical and contemporary theories, concluding that, "if there is in human behaviour an irreducible factor which may be known as temperament . . . its manifestations must be looked for along the lines of rapidity of natural responses and in duration of the disturbances produced. That is to say it is essentially a time factor."

Further discussion of such theories of temperament falls under the heading of *types*.

III. TYPES

From totally unrelated points of view and in the service of a variety of ends, innumerable *Typenlehre* have arisen. In its own way each doctrine seems to have some plausibility when taken in isolation and with respect to the special interest of the author. But since these authors seldom examine the meaning of the concept of type in general, nor the exact nature of their own usage in particular, each shows peculiar weaknesses when viewed logically or compared

with other doctrines. The clarification of the concept of types and the establishment of its legitimate place would be an invaluable service to the scientific study of personality.

A satisfactory review of type doctrines current in 1923 is given by Selz (248). He distinguishes between views which offer types of isolated functions (attention, memory, etc.) and those which pretend to classify the whole personality; these he further orders under the headings of structural, empirical and correlational types. Two years later Klüver (156) made a similar survey, focussing attention upon the work of representative writers rather than upon classification. Grundland (111) has reviewed several of the experimental studies of types, Oeser (211) covers many of the contemporary European schools, and Hoffman's survey (132) is fairly complete. For the older doctrines Roback's book (235) is invaluable.

The following classification of type doctrines contains all the main groups that can be detected in the field, though, as is only to be expected, several authors overlap into two or more divisions. Thus Kretschmer includes so much in his conception that he obviously belongs under both the *constitutional* and the *pathological* categories, and Achille-Delmas and Boll fall within the *deductive* and the *pathological*.

A. *A priori and descriptive*: (1) Literary, (2) Deductive.

B. *Biotypes*: (3) Energetic, (4) Constitutional, (5) Pathological, (6) Eidetic.

C. (7) *Statistical*.

D. *Qualitative*: (8) Gestalt, (9) Ideal.

A. (1) *Literary*. Throughout the history of literature from Theophrastus to the *New Yorker*, the writing of type-sketches has been popular. By means of arbitrary selection or *exaggeration à propos*, the writer draws an organized and *typical* picture. In general it seems possible to distinguish two principal methods in this field of "psychognosis."

(a) Theophrastus and his strict imitators pick out a dominant or crucial trait, and show how this central quality colors the everyday behavior of the individual; the result is the securing of a high degree of universality. The Chatterer among the ancient Greeks is the chatterer of to-day. The type is here the *dominant trait*.

(b) Other writers, following La Bruyère, modify this method in the direction of an actual portrait. More than one trait is depicted, but the skill of the author makes of the plurality an extremely harmonious picture or cluster of traits in which all that is distinctly

individual is deliberately eliminated. The patterns arrived at in these portraits are universal enough to serve as prototypes for whole classes of people. Giton, Phaeton and Theodacte are in our circle as they were in La Bruyère's.

(2) *Deductive*. The scientific standards of to-day do not permit exclusively deductive schemes. It would be impossible for anyone, for example, to secure a hearing with an entirely *à priori* construction, even though it were as grandiose as Fourier's. Up to the present century, however, this method was an accepted one for establishing types. A glance at the tortured variations which the four temperaments suffered at the hands of German and French writers of the nineteenth century will sufficiently convince one. This manner of logical approach can still be detected in the systems of Ewald (85) and Achille-Delmas and Boll (2) and others who attempt to force observation into categories which have only a vague empirical foundation. The neater the formulae for such types, the more danger there is of "discovering" individuals who possess the correct proportions of each basic ingredient.

B. *Biotypes*. To laymen and to psychologists the word type immediately suggests extrovert-introvert. Practically all of the biotypes can be reduced only too easily to variations on this theme. Hence it would be as well to consider Jung first.

(3) *Energic*. Psychologies which include the notion of a dynamic flow of energy readily invent types to correspond to the channels of flow of such energy. Thus Bain's "steam boiler" produced his cognitive, conative and affective types of "character," and Kempf's energetic stream followed characteristic paths in different people, nutritional, sexual or sublimational. Jung's conception is, of course, based on "outward or inward turning of the libido."

Guilford and Braly (112) have recently reviewed for the BULLETIN most of the literature on extroversion-introversion, and few of their references will be repeated here. In general it may be said that there are two marked tendencies in the current treatment of Jung's types. The first is to reduce the concept to a trait, normally distributed, making the large middle region of ambiverts more prominent. Considerable variation may be discovered in the definition of such a trait, but it has clearly come to stay, although Downey (80) points out that Jung considered extroversion and introversion as attitudes or mechanisms rather than as characteristic types of behavior, and Hendrick (128) objects to the psychologist's interpre-

tation of them as a static trait rather than as "a highly variable and complex mechanism."

The second tendency is to provide alternative renderings to Jung's dichotomy, either on a kinetic or an emotional basis. Guilford and Braly show that the division was by no means original with Jung, but has antecedent variations, notably in the primary and secondary functions of Gross. Heymans and Wiersma are among the chief proponents of these two functions; in addition they classify temperaments according to the criteria of activity and emotionality. In two recent papers (311, 312) Wiersma finds these fundamental distinctions in racial and occupational groups as well as underlying many psychological peculiarities of individuals. By means of somewhat crude though painstaking statistical treatment of ratings and questionnaires he seeks to connect huge numbers of traits (domineeringness, vanity, scrupulousness, etc.) into clusters around the particular types. Downey (80) identifies certain of her "rapid fire" or fluidic (primary function) will-temperament traits with extroversion and slow-cautious traits with introversion. An essentially similar distinction derived from industrial research is suggested by Mira (200) according to the degree of what he calls "the viscosity of attention." And Spearman (258) implies that his universal factor of perseveration is equivalent to introversion.

Interpretations of a more socio-emotional variety may be illustrated by Lundholm's (175) establishment of altrocentric and ego-centric tendencies as basic, an attempt similar to Decroly's (76). Perhaps the commonest assumption is that the extremes of extroversion-introversion correspond to syntonetic and schizophrenic psychoses, to use Bleuler's terms (39). Mauerhofer's (189) syntonetic (social minded) and idiotonic (self minded) or Wertheimer and Hesketh's (308) syntropic and idiotropic are alternatives. The so-called color (Fb) and kinaesthetic (B) responses to Rorschach's (238, 239) inkblots give rise to his "extratensive" and "introversive" types.

Pavlov (214) notes an "excitatory" and an "inhibitory" type of temperament in dogs, *i.e.*, those liable to disturbances of a cycloid or neurasthenic, and of a schizophrenic or hysteric nature, respectively. In between is a *central*, stable group which may be subdivided into the quiet and lively types. Thus he develops the usual dichotomy into a fourfold classification whose close connection with the humors he does not fail to stress: excitatory-choleric; inhibitory-melancholic; quiet-phlegmatic; lively-sanguine.

By following through Kempf's tension theory to adult social life, Frank (94) derives two types of adjustment; the "status" type of individual is one who, unable to solve the tensions of his early personal relations, is always intent on his own position with respect to society and the external world; the "objective" individual is one to whom personal and environmental relations come easily. Frank considers these as corresponding to introvert-extrovert, but more fundamental, physiologically and psychologically.

(4) *Constitutional*. Work on the physical make-up of individuals with its psychological counterpart is the most popular of all approaches to the problem of types at the present time, ranging from Berman's simple glandular types to Kretschmer's complex biopsychotic ones. Serviceable general reviews of the literature are given by Boven (43, 44), Burnham (63), Healy (123), Hoffman (132), Klüver (156), Podach (222), and Polen (223). This last reference contains a complete bibliography on Kretschmer's theory up to 1928.

Kretschmer's work (163) has been pivotal and has received wide acclaim from psychiatrists. In the main it has been speculatively modified and elaborated rather than adversely criticized, and his chief thesis, the correspondence between physique and types of psychosis, has been roughly confirmed, for example by Wertheimer and Hesketh (308) and Krasusky (162) who worked with children. The pyknic-cycloid correlation is found to be better than the asthenic-schizoid by Polen (223). Adler and Mohr (6) together with most American experimental psychologists are rather more critical, seeing valid distinctions between the types only for extreme groups. Miller (199) would relate a number of these doctrines of constitutional types to a simple neurologico-endocrine dichotomy, the sympathetico-vagotonic. Burt (64) shows that herbivorous-carnivorous, hypothyroid-hyperthyroid, objective-subjective, vital-mental, nutritional-nervous, extrovert-introvert, sthenic-asthenic, etc., are essentially the same. He posits a general factor of emotionality, superimposed on which are active and passive group factors to correspond to these types. One of the two divisions, the sthenics, is considered by Fowler (93). Innate masculinity and femininity characterize the theory of Sumner (269) in much the same way as in Weininger's well-known doctrine. Kroh (164) seems to have deserted the field of eidetic types and to be developing a typology more in line with Kretschmer's.

More strictly morphological work, relating features of physical

build to temperament and intelligence derives from the Italian schools, Viola, Pende and De Giovanni (*cf.* 235, 236). Both Gurewitsch (113) and Yizlin (318) classify pyknics and asthenics on the basis of fluent or awkward, harmonious or angular movements and handwriting. In this country, in addition to the theoretical discussions, there has been much experimental investigation giving positive though small correlations between personality tendencies and constitutional factors. Since they are well summarized by Guilford and Braly, we need not repeat here the results of the direct chemical attacks of Rich and of Furokawa, nor McDougall's autacoid theory of extroversion, nor Hunt's derivation of temperamental types from the degree of excitability and motility. Sheldon (252) finds some tendency for all-round bigness of frame to go with aggressiveness (ratings), but Bender (33) discovers no significant anatomical counterpart of ascendance. More than a dozen investigations of the correlation between asthenic-pyknic build and intelligence are summarized by Mohr and Gundlach (201); of these results, Sheldon's (251) $+ .136$ seems to be the safest (the asthenic being slightly more intelligent than the pyknic). With emotionality the evidence is conflicting, but with sociability (ratings) Sheldon finds a correlation of $+ .217$, with extroversion (several tests) Vernon (292) obtains $+ .28$.

Another popular variety of constitutional theory is the doctrine of racial types. Three very different approaches may be illustrated by the works of Porteus and Babcock (224), McDougall (178), and Madariaga (184).

(5) *Pathological*. As already mentioned, both Jung's and Kretschmer's views are largely based on psychopathological data, so that their extension to mankind in general requires the somewhat insecure assumption that the normal is always continuous with the abnormal. Many psychiatrists, however, enjoy ordering people, whether normal or institutional cases, under the genera of mental disease. Rosanoff's scheme (240) was an early one, while Bleuler's (39) syntonie (cycloid) and schizophrenic mechanisms have attained a considerable vogue. A large part of Menninger's recent popular book (198) is devoted to this task. He parallels the normal and the clinical types as follows: organically crippled (organic disease); stupid (hypophrenic); lonely (isolation); queer (schizoid); moody (cycloid); frustrated (neurotic); perverse (antisocial, psychopathic personality). Healy, Clarke and Kasanin (124) have made an empirical study of the types of personalities encountered in

abnormal children, establishing the unstable egocentric, the constitutionally inferior, the idiosyncratic and the psychotoid groups. A definite personality type among tuberculous women is suggested by the investigation of Mühl (205), while Notkin (209) proves that the so-called epileptic personality type in general occurs only among those who suffer from the disease in their earliest childhood.

(6) *Eidetic*. Related to the constitutional types are the biotypes of Jaensch and his co-workers (138-141). This fertile field has been reviewed by Klüver (158) and by Oeser (211), so that little will be added here. It is important to distinguish, however, between the problems of eidetic imagery and those of eidetic types; the validity of the prolific research concerning the former phenomena would not be in the least affected by scepticism regarding the enormous superstructure of types erected by the Marburg school. This view has been developed elsewhere by Allport (12).

Among the numerous types suggested by Jaensch and his associates are the following: the B-type, whose eidetic images are voluntary and controlled and whose physical make-up seems to suggest Basedow's disease; the T-type, whose images are uncontrollable and insistent, whose physical symptoms are analogous to tetany, with calcium as a specific agent for the reduction of the imagery. The mental life and personality of these types are affected greatly by their physical and imaginal peculiarities. There are also the BT-type and the TB-type, a TE-type (tetanoid-epileptoid) and BH-type (Basedow-hysterical). There is the Unitary type whose after-images, memory images and eidetic images are indistinguishable; an Integrated type, whose general eidetic tendencies are strong and whose mental processes interpenetrate, and the opposite Disintegrated type. There is the Synaesthetic type, given to symbolic thinking, and variations on all the above. One cries for a truce to types!

The work of other investigators on objective and subjective reading, perceptual and imaginal types, is too manifold to be considered here, although definite connections with personality as a whole are usually implied. Rorschach's synthetic-analytic (G-D) responses (238), Apfelbach's (22) masochistic-sadistic and similar subjective-objective dichotomies, possibly originated by Binet, should be mentioned. In general these do not correspond so well to Jung's extrovert-introvert attitudes as to his feeling and thinking functions. They receive their greatest elaboration at the hands of Hinkle (131), who definitely superimposes subjective-objective across the usual dichotomy.

C. (7) *Statistical*. Most of the type doctrines so far considered meet their Waterloo when they encounter the normal distribution. Since significant multimodality seldom occurs, there are only two ways in which the type concept can be treated by statistics.

First the type may be reduced to a unidimensional trait, with a frank acknowledgment that the majority of cases fall in the middle ground and only a few at the extremes. Extroversion-introversion has already been quite thoroughly absorbed into the bell-shaped curve. Or, to take another illustration, Baxter (30), attempting to differentiate temperaments on the basis of reactions to "speed" and "strength" tests, found some evidence of normally distributed general factors, but no bimodality.

An interesting variation of the statistical type is advanced by Giese (102); he believes that the great number of people in the average range of all distributions themselves constitute a type, and plans an elaborate research to establish the characteristics of the *Durchschnittsmensch* (D-typus), the 30 per cent to 70 per cent man, in respect to his habits of work, standards of living, mental capacity and personal characteristics.

Secondly, the two extremes of some quantitative variable may be used as a starting point, or two sociologically differentiated groups may be taken (*e.g.*, delinquents and normals, salesmen and mechanics, etc.). A large number of characteristics are then examined, seeking the traits that are constant and distinctive for each group. If a number of tests or other criteria agree in differentiating one group from the other (according to some such statistical method as the probable error of the difference), we attain, perhaps, the most valid type concept of any. This method is frequently used (*e.g.*, 16, 45, 67, 92, 95, 129, 169, 201, 202, 205, 207, 212, 213, 224, 246, 294, 311).

D. *Qualitative Types*. The two remaining categories of types are quite immune to statistics, being unaffected by the existence of mixtures or "middle cases."

(8) *Gestalt*. Lewin (171, 172), whose views are summarized in English by J. F. Brown (53), has made a bold attack upon the assumption that the statistical Gaussian curve deals adequately with the concept of types. His chief interest is in showing that neither regularity nor probability have anything whatever to do with *law*. Incidental to this principal argument appears the following unique conception of types. Types are natural happenings, and the fact of their existence has no relation to their frequency. Iron and gold are equally basic types, although the former is more common than the

latter. In the psychical world it is very difficult to isolate a type, for it remains enveloped in numerous *Restfaktoren*. In personality, for example, there may be ultimate types of will or activity that can only with great difficulty be discovered owing to the overlapping determinants of different kinds. Now on a distribution curve these elemental type happenings may be lost sight of. Take a simple example of sorting beans. A series is found to vary in dimension or weight between 10 and 40 units, and the distribution curve seems to be normal. As a matter of fact the assortment contains four *types* of beans, those which cluster between 10 and 19, those between 17 and 28, etc. In such a case, although the resulting curve is normal to the eye, the incidental variations in the types have obscured the truth, making a specious single variable appear in place of the four basic types. According to Lewin, then, a type should be considered as a qualitative event, however difficult it may be to isolate it. Statistical methods may actually obscure its existence.

Lewin further distinguishes two sorts of types, indeed two aspects of all psychological data. The first is the *genotypical* (*konditional-genetisch*), and it is this which he identifies with *law*. The other is *phenotypical*, serviceable only for the description of phenomena. In brief his argument runs that the same genetic sequence may give rise to diverse phenotypic sequences or configurations. In strict logic this proposition seems to violate the law of uniform causation, but it follows from a consideration of the impossibility of tracing the millions of determinants in the process of forming personality. For example you and I may both be introverted (a phenotypical fact), but I may be so owing to the effect of a sensitive temperament, you owing to bitter experience (two different genotypes); or under conditions of embarrassment (same genotype), I may become overbearing and you merely shy (different phenotypes). Something of the same distinction seems to be in the minds of Kronfeld and Klüver (154) in their division of types into "psychological" and "sociological." The former would be genotypes, the latter mere biosocial phenotypes.

A third important development is Lewin's conception of types of different orders. A type may be extensive or may be elemental. Smaller types exist within larger types; and one cannot generalize from one level to another; each must be observed with regard to its own properties, and each, if it is a genotype, is a "law." From this point of view it may be possible that we shall find types and sub-types in personality.

A final consequence in this view is the retreat in importance of the stimulus. The system or type upon which the stimulus acts will be the decisive feature in the reaction. Hence type becomes a sphere of determination, and such a concept, as we shall see later in our discussion of traits, is of great importance in the theory of personality.

(9) *Ideal*. A conception of type, totally different from all the others, which is philosophical in its origins, but exceedingly influential in contemporary German psychology, is that of the *Geisteswissenschaften*. It seems to have arisen from Dilthey's proposal that there are three fundamental ways of looking at life, the *Anschaungen* of the subjective idealist, the objective idealist and the naturalist. Dilthey was interested in both individuality and type. He was impressed, however, with the fact that certain features are commonly connected in man's mental outlook, so that the presence of one feature enables us to predict the presence of another. Whatever connections occur again and again he labels types. Combining this basic notion with Rickert's emphasis upon values, Spranger (261) decided upon six fundamental value-directions, each constituting a typical *Lebensform*: the economic, the theoretical, aesthetic, religious, social, and political. It is doubtful whether the author of any type-doctrine has thought out his position and defended his types as rigorously as Spranger.

The argument for the ideal type is well given by Klüver (157) and is further illustrated in Jasper's work (142). The latter writer does not use his ideal types as rigidly as Spranger. He distinguishes the sensuo-spatial (including the mechanistic, technical, historical, mythical), the psychic-cultural, and the metaphysical views of the world. It is important to understand that ideal types are not empirically derived. In spite of the title of the English translation of Spranger's book, they are not actually "types of men." They are rather "spheres of meaning," of which men partake in different degrees. Spranger has written primarily a *Wertlehre*; in his somewhat Platonic universe he finds this six-fold hierarchy of values. Men as *Träger* of these values necessarily manifest different personalities according to the proportions of the values which they bear. A purely theoretical or aesthetic man is merely a convenient, though improbable abstraction. Still, when these ideal values are reduced to the psychological realm, and applied to concrete personalities, the result is extraordinarily suggestive. E. Stern (263) has shown how this point of view sheds entirely new light upon personality, "In

what a man sees value, especially in what he sees the highest value of his life, that value in fact, which makes life important to him, that is what we must know, if we are to be capable of understanding his personality."

Although Spranger is aware of the aid which these ideal patterns give in the practical understanding of human nature, he refuses to take a genetic point of view toward the values themselves. Even in his book on adolescence (262) he persists in regarding the types as existing as ultimate forms of entelechy rather than as genetically conditioned interests or drives. Roback (235) suggests a genetic foundation for these types, assigning one or more of McDougall's instincts to each. The environmentalist could, no doubt, make a similar reformulation in terms of conditioning and the laws of learning.

Conclusions. It does not seem possible to discover any common denominator underlying all these conceptions of type other than the obvious fact that in whatever sense the concept is applied it always denotes groups of individuals who are in some way similar with respect to one or more essential characteristics. The dissimilarities between the concepts are more enlightening, however, than is this very general common characteristic. The most thoroughly examined conceptions of type seem to come from the statisticians, from the authors of the ideal types, and from Gestalt psychology. The inadequacy of most of the other doctrines may perhaps be clarified if we ask ourselves why it is that the enormous success of Jung's dichotomy in capturing the imagination of psychologists and laymen is only equalled by the failure of his four functions to be accepted with similar acclaim (*i.e.*, thinking, feeling, sensation and intuition). Is it that the original dichotomy is so much more valid than his subtypes? Probably not, for intrinsically the direction of flow of the libido is a more fantastic concept than are the four "functions." Possibly these functions are too "deductive" for general acceptance, but the phenomenon seems rather to rest on less rational foundations. Most authors who are interested in some special aspect of human nature express their interest in the convenient phraseology of types, and never consider the broader problem of their particular use of the concept. This method serves an immediate purpose, and is in this sense pragmatic; in so far as it leads others to criticize or confirm the doctrine it is also heuristic. But mental inertia seems to be at the root of most of the type doctrines. A dichotomy is much more readily grasped, understood, and applied than the permutations of

several functions. Dichotomies always have been popular, they facilitate intercourse and are effortless for our comprehension; witness Appolonian-Dionysiac, subjective-objective, toughminded-tenderminded, sensory-motor, primary function-secondary function, masculine-feminine, pyknic-asthenic, jigger-goon, extrovert-introvert. These simplifications are in part the product of lazy thinking and in part a token of desperation among lecturers. But progress will be made in proportion to the substitution of more adequate conceptions for these clichés. In this sense the type doctrines can be considered merely as propaedeutic to more adequate formulations.

IV. GENETIC STUDIES

The development and the social behavior of children naturally have a considerable bearing upon the formation of personality. Only a few illustrative references will be given here, since there are available several useful reviews of the literature by Shuttleworth (255), Baldwin (27), W. I. and D. S. Thomas (278), and H. E. and J. C. Jones (145). Probably the best cross-section of the leading genetic investigations and theories, as carried on all over the world, is to be found in the Proceedings of the First International Congress of Mental Hygiene.

A. *Methodology*. (1) *Observational Studies*. One of the most marked advances in this field consists in the development of the controlled observation of young children. Important techniques and results are reported by C. Bühler, Hetzer and Tudor-Hart (56), Bühler (58), Rugg, Krueger and Sondergaard (241), Pratt, Nelson and Sun (225), Zoepffel (327), Goodenough (106), and the Sheremans (253). Cf. also Section V.

(2) *Case Studies*. No detailed mention need be made of the hundreds of descriptive studies appearing in psychiatric, sociological and popular journals, but the general trend is important. Young's summary of methods of investigation (322) includes a useful account of the historico-genetic, sociological and psychoanalytic approaches. Fearing (86) has reviewed psychological and psychoanalytic studies of historical personalities. The case-study method is also considered by Burgess (61).

In certain branches of this work there is a considerable tendency towards superficiality and psychologism, for example in biography such as Herzberg's study of great philosophers (130). Like so many "psychobiographers" he assumes that great thoughts "naturally" flow from organic inferiority or economic incompetence. A better

rounded approach to the personalities of productive people is offered by Plaut (221), whose book contains, besides theory, short autobiographies of many creative persons. One might also mention especially Stern's analysis of an adolescent's diary (265), and Sell's (247) exhaustive study of a twelve-year-old boy, in which he employed nearly one hundred psychological tests. An autobiography which has special significance for psychologists is that of Leonard (170). This book has been discussed as material for teaching by G. Allport (17) and as a clinical problem by Taylor and Culler (275).

In general, unsatisfactory though the case method may be, many writers feel that the concrete individual has eluded study by any other approach. In the future there will undoubtedly be attempts to standardize the case-study in some way which will reduce its dependence upon the uncontrolled artistry of the author. Its chief merit is that it can include a great deal of material, and that it can indicate something of the relations between the various factors which are excluded by the more static and objective methods. For it attacks both aspects of personality, the genotypical and the phenotypical, as distinguished by Lewin; it is both history and portrait.

(3) *Environmental and Sociological Studies.* There are innumerable investigations of environmental conditions which implicitly or explicitly are assumed to play their part in the formation of personality. Giese's (101) statistical study of the external factors surrounding 10,000 men of public life is an ambitious illustration. The effect of the home, including the attitude of parents, has been an especially popular field; cf. Blanchard (38), Foster (91), B. Glueck (105), Shaw (250), Smith (256), Taft (273), and Young (321).

The environment is, of course, a favorite stamping ground for sociologists whose interest to-day in personality is very lively. A plea for the study of personality through the medium of environing culture is made by Burgess (59, 60, 61); faith is placed in mores rather than in neurones. Shaw's ecological work on delinquency (249) is sometimes acclaimed as a model for the "situational" approach to personality. Now there is little in such studies that concerns the concrete *psychological* phenomenon which is the object of our interest. By making the cultural group the principal avenue, and by exploring the avenue rather than the destination, the sociologist is in reality diverting attention from personality to its mere cultural setting. G. Allport (18) has criticized this trend, and

Hankins (117), himself a sociologist, has pointed out that sociologists forget that the organism itself determines the direction, and supplies the energy for the shaping of personality. The situation merely releases one of several courses of development. On the misuse of the term "personality" in sociological writing, see the review of Young's book (325) in this number of the BULLETIN.

B. *Results for Theory.* (1) *Heredity.* If a general impression may be recorded, it seems that the tide has turned toward greater faith in the innate determination of traits. Nearly all the observational studies, cited above, agree that the foundations of personality (individual differences in respect to motility, prevailing mood, social responsiveness, etc.) are readily detected in very young children, in Zoepffel's investigation even as early as the twentieth day. Gesell's work (100) also stresses the unalterable nature of native temperament, and the rôle of maturation.

Literature on the inherited bases of personality is covered up to 1923 in the review of Peters (217), and instincts have been reviewed by C. Bühler (57). Considerable interest is shown by German investigators in the personality of animals; the field is well surveyed by Schwangart (245), while Burlingame (62) has summarized work on the inheritance of temperamental traits in animals. Kelley (146, 148) believes it possible to determine the relative proportions of nature and nurture in personality. May and Hartshorne (195) think that whatever arguments are valid for the native determination of sibling resemblance in intelligence apply as well to sibling resemblance in the complex factor of deceptive behavior.

(2) *Integration.* Among the theoretical problems connected with the genetic approach, none is more important than the nature of integration. Studies which trace integration from its lowlier manifestations up to the human personality than those of Child (69), Young (320), May (190), and Sherrington (254). The last of these authors seems to complete his basic theory of 1906 when he states in 1922, "the normal action of the mind is to make up from its components one unified personality."

Coghill (73) is critical of the concept of integration: "The development of behavior must be regarded not as a synthetic process, but as a process of analysis of a total, primarily integrated and constantly expanding pattern." Differentiation within a whole rather than binding of elements together is also the kernel of the Gestalt theories. Stern has said *dissociation* is a more valid law of personality than *association*.

Whatever the theory of the unification of personality may be, there is a marked tendency to use the concept of integration as representing normality, maturity and mental health (*e.g.*, Burnham, 63). Künkel (167) considers progressive integration in personality as equivalent to the absorption of each single experience (*Erfahrung*) into adequate preëxisting experience-systems (*Erlebnis*). Defining integration as the absence of frequent alternations of contrary traits in essentially similar situations, Brooks (52) regards adolescence as usually unintegrated. The chief agencies for producing integration Bernard (36) believes to be the social processes of suggestion and imitation.

Less enthusiasm is shown by MacCurdy (176), "With the perfection of reaction patterns achieved during advancing years, personality becomes better integrated, but a heavy price is paid; imagination fails and intellectual sterility ensues." Gates (99), sensing the same dilemma, meets it by distinguishing between integration and inflexibility; "personality may be well-integrated and yet flexible. The integrated, inflexible personality is one which is always the same." This view is much the same as Dewey's (77).

V. EXPERIMENTAL METHODS

Thorndike's fundamental axiom (280) that "Whatever exists, exists in 'some amount,'" is often quoted and widely respected. But its effect (or the effect of the quantitative type of outlook for which it stands) has been insidious in the study of personality. Our discussion so far has shown that the chief problems of personality are seldom amenable to quantitative treatment, so that most of the literature on measurement contributes little to the theory of the topic. That experimental approaches in recent years have far outgrown an adequate philosophy and theoretical background, especially in America, is obvious, however gratifying they may be as signs of an increasing interest in the vocational and applied psychology of personality. A general critique of testing methods and results is badly needed. Almost the only instances of such are May and Hartshorne's first annual summary (193) and W. I. and D. S. Thomas (278). Watson's book (300) and Young's articles (322, 323) are also useful. But these are already out of date. Here we can only touch on a few of the more important trends which do not usually fall within the scope of these articles or in May and Hartshorne's later non-critical surveys (194, 196).

First there seems to be an increasing dissatisfaction with the more artificial pencil and paper or laboratory methods, together with the realization that, while the test situation should be controlled, yet both situation and response should be as natural as possible (*cf.* Hartshorne and May's excellent criteria for testing, 120). Apparently the more strictly objective the approach, the less normal and spontaneous are the testee's reactions, and therefore the less significant the results. There has been too great a tendency to ascribe such poor results to lack of consistency and integration in the testees rather than to defects in the techniques (*cf.* Gilliland, 104). Bain (26), gives an analysis of the difficulties in ensuring correspondence between verbal responses to written tests and behavior in real life, while Johnson (144) argues that we are guilty of homeopathic magic in that we consider our test results to be *equivalent* to the actual psychological functions that we are attempting to measure, instead of merely "*equivocant*." The need for consideration of the subjective situation in testing, not merely the objective conditions, is emphasized by E. R. Hamilton (115) in a brilliant analysis of the theory of mental measurement (which applies equally to personality testing). Vernon (291) criticizes the more objective techniques from the same standpoint. Bronner and her associates (51) advise against mechanical testing and the basing of conclusions on specific numerical results, rather than taking into consideration all the exogenous and endogenous factors which influence the individual's responses.

Many investigators claim to reach results of greater predictive value by combining the clinical and the objective approaches. Beck (32) has demonstrated for the first time in this country the possibilities of the Rorschach ink-blot method (238, 239). Burt (66), Porteus (224), and Vernon (291, 292) find that qualitative deductions from performance test situations, in spite of the difficulties of validation, reveal much about personality. Viteles (295) and Anderson (21), in the industrial and vocational fields, regard tests and ratings as incapable of giving information about the personality as a whole. Such results need to be synthesized by a more subjective approach. The latter writer offers statistical proof of the superiority of a psychiatrist's guidance to that of an interviewer using standard tests. Young (324) pleads for the combination of the longitudinal or historico-genetic method with the cross-sectional studies.

Henning (129) has developed clever apparatus and instruments for bringing out individual differences. In general two or more subjects work together, and he has designed some 80 tests to disclose

which of them shows the greatest initiative, honesty, recklessness, tolerance, helpfulness, etc. His methods usually lead to rank-orders for a group of subjects, for which he claims a high reliability. Validation is accomplished by the group difference method. Meltzer (197) distinguishes optimists from pessimists by individual differences in the tendency to forget unpleasant experiences or pleasant experiences.

A remarkable number of studies with children (similar to those mentioned in Section IV) have led to promising results when perfectly natural and spontaneous behavior was observed; certain significant aspects of this behavior being expressed indirectly in quantitative terms. This work has been validated by means of ratings, by the large individual differences that were recorded, or, best, by the internal consistency or reliability of these differences (*vide infra*). Typical studies are those of Marston (187) and Newcomb (207) on extroversion-introversion; Berne (37) on obedience, coöperativeness, etc.; Wagoner and Armstrong (296) on self-dependence, reliance and care in details; Ball (28) on emotional instability; Cushing (75) on perseveration; D. S. Thomas and associates (276) on talkativeness, laughter, social resistance, "self, social and material" activities. Thomas calls her new techniques "experimental sociology."

Since 1925 the situation with respect to *validation* has changed considerably, although May and Hartshorne foreshadowed many recent advances (193). One should mention first a new and fruitful method which is based on qualitative data and is akin to the German "interpretative" approach rather than to the usual quantitative correlational method. Hendrick (128) suggested that test results should be compared with case studies drawn up by experienced clinicians, whose personality diagnoses would clearly be superior to those of the average rater. Travis (283) employed this method, claiming 80 per cent agreement between his test findings and psychiatrists' descriptions. Gorham and Brotemarkle (109) compared case studies which were written by outside observers with similar studies based on the interpretation of certain test results, but were unable to express the degree of agreement numerically. Vernon (292) tried to fit or identify unnamed qualitative case studies to the personalities of his subjects, with fair success. Similar also are Hartshorne and May's "Guess Who" method (121), and the matching techniques of certain German investigators such as Arnheim (23) and Wolff (315, 316). Another development, which also attempts to

include something of the "form-quality" of the total personality, is work, not with single traits or qualities, but with several traits simultaneously. Thus if two sets of tests or ratings provide two scores for each individual on a number of traits, then the order of such traits within the individual, according to one set of tests, can be statistically compared with the order according to the other set. Allen (8) and Flugel and Radclyffe (89) have applied this to Pressey X-O and an emotional questionnaire, Vernon (292) to tests of interests and to personality traits.

Although these methods are only in their infancy, it is possible that they will develop considerably in the future owing to the perennial difficulties of the usual correlational methods. The chief disadvantage of statistical treatment which involves reliability and validity coefficients, and the like, has been pointed out by Young (323), namely, that it is essentially dependent on the assumption that the variables or functions which are being measured are homogeneous, unidimensional and distributed according to the normal curve. This cannot but distort the actualities of human nature. A further criticism which applies to all types of testing and validation is voiced by Kelley (147) and May (192), who prove the vital importance of taking into account the heterogeneity of the groups which are tested. A correlation, between a test and ratings let us say, is meaningless unless we know something of the diversity of the population; external factors such as maturity, intelligence, race, age, sex, and cultural background may raise such a coefficient from a very low to a very high value. Kelley therefore discusses the most desirable degree of heterogeneity to be aimed at; Spearman (260) also gives an account of this and other pitfalls.

With these cautions we may consider the chief correlational methods. There has been a striking decline in the popularity of ratings now that it is realized that halo and other errors may falsify such estimates as much as objective test records are distorted by errors of observation. Hartshorne and May (121) show that tests will cover part of the "ground" (*i.e.*, of the quality that is being measured) and that ratings will cover part, but that the two do not necessarily overlap. Thus instead of regarding ratings as the ultimate standard of verification, they are the "reputation" of the ratees among the raters, which may be treated as one valuable type of approach, on a par with other more objective approaches.

Validation by differentiation of groups has been used to considerable effect in finding, for instance, how well tests will differ-

entiate delinquent from normal children (Cady, 67; Henning, 129), manic-depressives from dementia precox cases (Neymann and Kohlstedt, 208), salesmen from mechanics (Freyd, 95), prejudiced from fairminded (G. B. Watson, 299). But the most important development is what May and Hartshorne (193) called validation by sampling, now known as theoretical validation or internal consistency. Instead of trying to find one test for a particular trait, a battery of tests is employed, perhaps following in this way the accepted principles of intelligence testing. We may consider these subtests as measuring different aspects of some trait, but more often no such assumptions are made as to the psychological function in question. Hartshorne and May's character education studies are the chief example (120, 121, 122). Here the qualities to be measured are practically defined as the average of the results of an infinite number of tests. Any one test (or set of ratings) covers part of the ground and agrees to a certain extent, known as its theoretical validity, with the sum of an infinite number of other tests. Obviously the more tests we have, the nearer will our composite measure, based on the battery, approach to the ideal infinite number, and the more adequate our sample. Moreover, the more closely the components of the battery agree together, the higher is said to be their internal consistency and the higher will be their theoretical validity. Thus Hartshorne and May's nine tests of dishonest behavior intercorrelated, on the average, to $+.227$. When corrected by means of the Spearman-Brown prophecy formula, the reliability or consistency of the total battery was $+.725$. This figure represents the expected correlation with another battery of similar tests, and its square root, $.851$, is the expected correlation with an infinite number, *i.e.*, with the criterion. Now a corrected consistency of $+.90$ would give us a validity of $+.95$, which is the highest figure that we are ever likely to attain. By the application of the prophecy formula we can therefore find how far our tests go, that is, how near they approach to the desired $+.90$. To raise $+.227$ to $+.90$ would require stepping up 31 instead of 9 times; thus the battery should include 22 additional similar tests. Or in other words, Hartshorne and May's tests of deception cover $9/31$ of the total ground. Similar procedures were applied to persistent and coöperative behavior. The validity of any one subtest is then its correlation with the sum of the others in the battery, since the battery is the nearest we can get to the perfect standard of the infinite number. Similarly Vernon (292) applied some 15 hours of varied tests and found that 55 of the resulting sets

of scores could be grouped into nine separate composites. Each composite of five to eight different subtests "hung together" or showed a sufficiently high internal consistency to be used as a criterion or standard for some quality or trait. The average corrected consistency of these composites was $+.65$, so that their validity was $+.806$ (without resorting to attenuation corrections). Moreover, such composites as were theoretically valid did represent remarkably well the subjective idea of the traits in question, according to a clinical analysis of the personalities tested.

It seems probable that whenever, in the future, objective tests are to be used for practical purposes, this procedure will be adopted, rather than the old method of trying to find one test for one specific trait. Many previous investigators have approached the same principles without reaching the level to which Hartshorne and May have brought it. Heidbreder's (125) scale of extroversion-introversion was, perhaps, the first direct application of this device. She considered the degree to which each item in her scale predicted the composite result based on over fifty items. Later this method was extended to the "normal inferiority complex" (126). By reason of this "hanging-togetherness" of the component items, she regards introversion and inferiority as "empirically consistent traits." (May would say "theoretically valid," since he employs the term "empirical" to describe the older rating and group differentiation methods of validation). If the test or the composite battery is internally consistent, then it is considered as valid whether or not it agrees with other criteria such as ratings. Allport's A-S test (15) also depends on this criterion of self-correlation rather than on its accord with ratings. The 233 items of the Thurstones' neurotic inventory (282) are similarly found to be positively associated so that the authors consider its internal consistency as sufficient validation. One should point out, however, a certain fallacy in these applications of the device, namely, the fact that the testing technique introduces a constant error or halo, as it were, into each item, so that the resulting interagreement is to a degree spurious. Hence it is better, when possible, to try to measure the quality by a variety of different techniques, selecting those which intercorrelate suitably, in much the same way as in an intelligence test battery.

The theoretical corollaries of these and other results where batteries of tests were used will shortly be considered in detail. A far more elaborate procedure which might be profitably applied would be Spearman's (258) and Kelley's (147) techniques; for there is

clearly a close connection between this average of an infinite number of tests and the former's universal factors or functional unities. As a matter of fact, the only additional advantage for personality study of Spearman's tetrad difference method is that it enables one to establish whether such factors are independent of *g*. Kelley, however, stresses the importance of considering many possible traits or factors in conjunction with one another, so as to try to cover most of the personality, instead of dealing only with *g* and perseveration, or *g* and oscillation, etc. His pentad difference method was developed for this purpose. The Robinsons (237) also propose interesting statistical criteria for the grouping of abilities or factors.

A possible objection to these ideas is that personality traits are not likely to be so amenable to this statistical treatment as the comparatively simple and definite abilities with which statisticians have worked. There is a widespread fallacious belief that measurement of personality is in a state to-day analogous to that of intelligence testing twenty-five years ago. But according to present indications, it is most unlikely that sufficiently discrete, homogeneous variables will ever be established; traits are too fluid, too fundamentally and uniquely dependent on the "form-quality" of the total personality (*cf.* Allport, 11). Hence it is that such "orectic" general factors as have been claimed by Spearman's school, *p* (perseveration), *o* (oscillation), etc., turn out on closer consideration (according to Vernon's experiments, 292, and indeed according to Spearman's own results) to be relatively independent motor qualities which bear very little relation to the personality traits of introversion and emotionality to which they are assumed to correspond.

Cushing (75), however, established a trait of perseveration on the basis of *natural* test situations, using both the average intercorrelation or internal consistency and the tetrad difference techniques. Apart from Webb's well-known study (303), McDonough (177) seems to be the only other investigator to have followed out Spearman's or Kelley's suggestions. By this means she discovered general factors of will, cheerfulness, sociability and emotionality. But since she and Webb were dealing almost solely with correlations between ratings, it is impossible to say how far halo affected their figures; so that their results would seem to represent interrelations between the traits in the minds of the raters rather than in the personalities of the subjects. Nevertheless it is in this direction that some of the most fruitful developments would seem to lie. What is most needed is that tests as natural as Thomas', Marston's, Cushing's, etc., and as

thorough as Hartshorne and May's should be applied to every aspect of personality, and that these results should then be treated by some such method as Kelley's. We should then begin to know whether objective experimental methods really throw much light on the psychological nature and structure of personality.

VI. CONSISTENCY AND TRAIT

The applications of experimental methods have brought to the fore the problem of consistency or spread of personality traits. Now it is unquestionably safe to say that personality, as it is ordinarily understood, depends upon some sort of consistency in behavior. A taxicab in which all sorts of aims, purposes and designs are carried to their destination and discharged, each one independent of the others, has no personality. A man is not like a taxicab. We can and constantly do predict correctly that his behavior to-morrow will fit in with what we know of his personality to-day. Hamilton (115) calls this "the principle of wider application," and recognizes that the amount of this wider application or spread is a problem of fundamental importance. Some psychologists admit more consistency, some less. A further difficulty, which we shall consider later, arises when we ask if the apparent consistency is really an attribute of the personality observed or merely of the observer's oversimplified social judgments.

The principal source of disagreement is known as the issue of specificity *vs.* generality. Common sense postulates the latter, experimental studies give results which are interpreted in either direction, often according to nothing more than the inclination of the author. The usual procedure is for an investigator to set out to examine some alleged "trait," by determining whether testees respond in a consistent fashion to several tests designed to measure this hypothetical trait. When the intercorrelations are low, the investigators have often declared the trait non-existent, and the testees' behavior to be merely specific responses to each individual test that is applied. Neither speed of decision nor confidence are general characteristics of a person's behavior, according to Trow (285, 286), nor is speed of work, according to Dowd (78). Hartmann disposes of accuracy (118); Guthrie (114), Hovey (135), and Weber and Maijgren (304) of introversion, Cowley of leadership (74), and Gates (98) of general maturity, except in so far as produced by the halo influences on the traits that were rated in his study. Working with the psychogalvanic reflex, Wechsler and Jones (305) find that

different types of emotional stimuli evoke comparatively unrelated deflections. By far the most important representatives of this outlook are Hartshorne and May, who not only obtain low intercorrelations between some 37 tests of various aspects of "good character," but show that even these figures are due largely to the influence of heterogeneous factors such as intelligence or classroom morale, rather than to consistency within the individual subjects (*cf.* Hartshorne, 119). They conclude (120) that, "honest and deceptive tendencies represent, not general traits, nor actions guided by general ideals, but specific habits learned in relation to specific situations which have made one or the other mode of response successful." Similar conclusions apply to persistent or coöperative behavior (121). Where the tests do agree it is said to be due to the presence of overlapping habits, since a direct relation is found between the number of objective elements common to the two test situations and the size of the correlation between the two sets of results. Again they find that different situations may elicit very different amounts of deceptive behavior in the same children, so that no homogeneous, organized and unified trait can be demonstrated.

It should be realized, however, that the average intercorrelation between many tests in such studies seldom approaches zero; more often it lies between about $+ .25$ and $+ .45$. Other investigators who obtain figures of much the same magnitude consider their results as evidence of generality rather than specificity. For example, Kennedy (150), measuring various simple and complex mental reactions, takes her average r of $+ .45$ to prove "a tendency for a person who is quick in one task to be quick in another." Unlike Peak and Boring (215), she finds this "trait," which she calls "irritability," to be independent of intelligence. Vernon (292) confirms both these results. MacFarland (183) likewise finds that ability in respect to speed is an individual trait which is a fairly constant characteristic of a person's mental behavior, and Braun (47), who used tapping, lifting weights, walking, writing, copying, reading, and singing as tasks, discovers a consistent personal tempo among his subjects (average $r = + .44$). Hübel (136) shows that speed of association, movement and reaction is a single factor, but that it is independent of speed of fluctuation of attention.

Cushing (75) obtains an average r of $+ .42$ between five tests of perseveration, defined as "the tendency of an individual to continue in a given mode of behavior where external pressure for continuance, in so far as is possible, has been reduced to a minimum."

Spearman's p , though differently conceived, is nevertheless claimed by him as a general factor. Spearman (259) interprets the fact that correlations between mental qualities are seldom either perfect or zero as refuting both the doctrine of "faculties" (*i.e.*, complete generality) and Herbartianism (complete specificity). Heidebreder's, Thurstone's and Allport's results with respect to the internal consistency of their tests have already been noted.

Goodenough (106) making repeated observations in the manner of the "time sample" on young children, found reliabilities ranging from $+ .32$ to $+ .87$, the highest being evidence for consistency in leadership, general activity and dramatic play. W. I. Thomas (278), commenting on similar experiments in which one child was found to be leading or dominating in 95 per cent of play situations, another, pleading or subordinate in 95 per cent of situations, writes, "When we consider the great number of combinations from which these computations come, the consistency of behavior is amazing." Similar conclusions have been arrived at in respect to talkativeness. Working with nervous habits, Olson (213) finds that "a score in one category (oral, nasal, ocular, etc.) has some validity as a measure of habits in other categories." His median r is $+ .48$. Marston (187) obtains good agreement among tests involving various social situations indicative of extroversion-introversion.

At what might be considered higher levels of personality, several investigators find that consistent *patterns* of traits are associated. For instance Schwegler (246) shows that introverts have slower word association responses, fewer contrast responses, less free production of inkblot interpretations, slower movements, less richness and variability of emotional output, etc. Oliver (212) obtained similar differentiations with many tests. Downey's grouping of Will-Temperament subtests according to extrovert and introvert types has already been mentioned, while Oates (210) finds empirically that many of these subtests fall into sthenic and asthenic emotionality groups. Jersild (143) discovers "desirable" traits to be associated. Analyses of radical and conservative individuals have been carried out by Moore (202), Vetter (294), and Allport (16). Freyd (95) established various interlocking characteristics of the socially-minded as opposed to the mechanically-minded; Lehmann and Anderson (169) dealt with social and solitary boys; Foster (92) with jealous children; Goodenough and Leahy (107) with only children, first born, etc. Such research leads, of course, to the conception of

types. For if associated traits are regularly discovered, the natural outcome is some statistical doctrine of type (*cf.* Section III, C).

Certain further generalizations arise out of these diverse experimental conclusions. Trow (286) suggested that some traits might be found to be more liable to yield consistent findings than others, or else that some persons might turn out to be more consistent than others, either in the various tests of one trait, or in tests of any trait. Since then, Hartshorne, May and Shuttleworth (122) have demonstrated large individual differences in the degree of what they call integration, *i.e.*, in consistency of objective scores on a large number of tests of honesty and the like. The actual average intercorrelation, they found, was due to 41 per cent of the children being more consistent than chance scores would allow; the others having negligible or even negative degrees of integration. Their measure of integration correlated highly with total honesty. This integration is explained as representing the amount of interpenetration of the specific habits, a view also put forward by Garrett (97), who considers a well-integrated personality to be "one in which the various habit systems show no marked disagreements." By means of statistical treatment of ratings for work, play, social and other patterns of habits, he attempted to devise indices for this internal harmony, but was not so successful as Shuttleworth.

Secondly it will be remembered that Hartshorne and May themselves showed us how to combine tests into suitable general factors, thereby obtaining measures of extremely high validity and predictive significance. This seems to be a justifiable statistical procedure, and many of the specificists would be surprised to find how promising are their low average intercorrelations when considered in this way. Surely it is largely due to the fact that they have isolated relatively meaningless or artificial bits of behavior, which could only be expected to bear little relation to other isolated bits, that has led to their view of human nature as consisting of such bits. The traits we talk about in everyday life are general types of behavior, and it is their artificial dismemberment into elemental test responses which inevitably produces these apparently disconnected and unintegrated results. A very good example is Newcomb's investigation at a boys' camp (207). By recording on a prearranged schedule the daily behavior of 51 delinquents, he was able to calculate the consistency for 26 items, also for assumed traits or groups of items, and for type (extrovert-introvert). The data are not easy to understand, but he seems to have found the greatest inconsistency in respect to

the single items (average $r = +.147$). In nine more general "traits," 45 of the 51 boys were consistent with respect to one or more, though only nine were consistent with respect to three or more. As regards type, these traits tended to be associated to an extent expressed by an average r of $+.374$. On statistical as well as on psychological grounds, this might be expected, and Newcomb seems unduly pessimistic. (For instance if he had correlated any one of his 26 items with the sum of the other 25, he would have obtained, on the average, a coefficient of about $+.63$, which we may call its validity with respect to type.) Newcomb's explanations of the apparent degree of specificity in his results are illuminating: first that his boys were young, suggestible and plastic; secondly that no two incidents recorded were ever precisely identical, that is to say that various unrecorded external and internal factors affected each particular response, so that, thirdly, it was the fact of trying to force these responses into *a priori* logical grooves which split up the natural and relatively organized wholes.

It is obvious that if an investigator prepares a schedule or scale according to his preconceptions concerning the nature of the trait to be investigated, then his results can at best only describe the individual's consistency with respect to this arbitrary criterion. Other experimental evidence tends to confirm this interpretation; for as a rule the greatest generality seems to appear, not when highly controlled techniques or the more artificial pencil and paper techniques are employed, but when the experimental situations are more natural so that the testees' true consistency and normal traits are able to emerge, undistorted.

We may turn then to the more theoretical discussions of the topic and of the nature of traits. Doctrines of generality are clearly favored by Kelley (147), Prince (227), Gilliland (104) and Allport (19). On the other hand, Hartshorne and May are by no means alone in their support of specificity. Folsom (90) puts the view crisply when he writes, personality is "a complex of millions of specific habits." Witty and Lehmann (314), impressed by the low correlations between tests of nominally the same trait, regard "character" as the result of specific conditionings. A doughty opponent of traits is Symonds (271). Considering Webster's definition of tact unpsychological, he has analyzed (272) "the ready power of appreciating and doing what is required by circumstance" into "a host of subtle habits." "If tact does to any extent, rest on a basis of habit, one ought to be able to test out these habits and catalogue them." In this

investigation he obtained 1,173 items which might be considered instances of "tactless" behavior (thereby, incidentally, appearing to analyze away the essence of tact): for example, belief that oneself is superior to others, chewing gum in public, and wearing soiled clothes. Thus the specifist would not ascribe the charming quality of tact *in toto* to even the most gracious hostess; her failure to commit specific offensive acts is for him the whole of the story.

For Symonds the unit of personality is the specific habit (270). Now this habit may be activated by any recurrent element in the original stimulus situation, even though this element occurs in a new context. Behavior which results from this process of reintegration may resemble a trait, but should more accurately be called a "confact." Thus, a boy "learns to take off a specific cap when coming in a specific door and in the presence of his mother. But in time he may take off his cap or hat or whatever he has on his head when entering any door, in any house whatsoever, whether or not in the presence of a person." Although subtle and receding stimuli evoke the response, yet the response itself does not show any generality, in this case "chivalry." This view illustrates the lengths to which the specifist will go, although the experimental findings, as described above, do not necessarily seem to support it.

What then is a trait of personality? In the review of 1927, Allport (13) attempted to collate existent views and to classify them under representative definitions. First the statistical approach, including both specificity and generality, stresses the view that variables which are to be accepted as traits should be, as far as possible, independent of other traits. Secondly the genetic approach implies that traits are general and biophysical in their nature, integrated from simple reaction patterns or conditioned reflexes. Thirdly the dynamic approach considers generic traits as systems of energy in themselves, and either as modified instincts, like McDougall's, or as self-sustaining *Gestalten*, or developed from the specific and mechanical adjustments of the infant (*cf.* Gillespie's genetic-dynamic outlook, 103). Typical of the latter view are the psychoanalysts' traits (see, for example, Abraham, 1), or the drives of the individual psychologist (*cf.* Adler, 4, 5, or Wexberg, 309). While these cannot be discussed in detail, yet one should note that the first approximately scientific evidence for the validation of some of Freud's characterological speculations has recently been presented by G. V. Hamilton (116).

To these three conceptions should be added the view, arising out

of the problem mentioned at the beginning of this section, namely, that traits are nominal fictions or artifacts. Weiss (306) regards traits as instances of "biosocial equivalence"; the observer takes ready-made social judgments and applies them to really unrelated acts. "Benevolence," far from being a general trait, is merely a symbol applied to many specific behaviors which, socially considered, comprise a single ideal. This view appeals also to the specificists such as May (190). Snobbishness, he states, is not so much something possessed by the individual as something inferred by society. As a stimulus the individual "touches off typical reaction patterns in his associates" and these patterns are called traits. Thorndike (281), Perrin and Klein (216), Leary (168), and Rexroad (232) would seem to be representative of this view which objects to regarding traits as existential, biophysical entities. A defense of the opposite view is given by G. Allport (19, 19a).

Leaving the controversial field of traits, the still higher unities in personality have been relatively neglected by experimentalists. The method of approach is very different, since it aims at discovering patterns of uniqueness rather than normal distributions or types. Regularities of co-variation and the comparison of individuals with one another vanishes here; instead the crucial and highest integrations of a single, isolated personality are sought. It may be instructive, as Köhler writes (160), to see a hundred hearts together, but functionally a person's heart has much more in common with his lungs than it has with the hearts of other individuals. The problem of unique consistency is like seeking the particular relations which exist within a single organism between the heart, lungs and vascular system.

The most suggestive experimental approaches to this problem are those of Arnheim (23) and Wolff (315, 316). The latter finds it possible to match voice and script of unknown authorship, so that they make a consistent picture, with a thumbnail description of the personality. His method is ingenious and consists in comparing the results obtained with chance. With physical features the harmony does not seem to be so marked.

It is, of course, the *Geisteswissenschaften* that have placed emphasis most squarely upon the "undivided personality" through the integrative concept of subjective values, but the writing of W. Stern (266) shows more clearly the disadvantages of analytical and test psychology. The case history as we have shown is an endeavor to escape these disadvantages, and psychoanalysis has

usually succeeded quite well in keeping the entire personality in view, Alexander (7) putting even additional weight upon the *total* personality. Gillespie (103) feels the need for an account of the organization and integration of the elemental factors elicited by the psychiatric diagnostician. The "style of life" is considered by Adler (5), and discovered to exist as early as the fourth or fifth year, and to consist in the strategies adopted by the individual for conquering his inferiority. The whole movement for "essentialism" in the school of *Gestaltbiographie*, discussed by Hook (134) may be taken as an illustration of the desire to depict the consistency of personality at its highest level.

On the other hand, Lewin (172), although a Gestalt psychologist, cautions us, "Just because we wish to place the problem of the whole at the front, we shall have to beware the tendency to make the wholes as spacious and inclusive as possible." There are systems *within* personality which are firm and unified, but the entire personality does not necessarily comprise a single Gestalt. Ichheiser (137) and Downey (81) treat the causes and consequences of overestimating the unity of personality.

Here we are trespassing on psychologically a distinct problem—*how we know people*. This review has dealt almost entirely with personality in the objective sense, that is to say, with the actual nature of personality as approached by experiment and by theory. The important problems of the perception and understanding of personality the writers hope to present in a later review.

The present survey has demonstrated that there is a marked increase of interest in the field; that most of the present work is in line with standard psychological research, simply extending and applying familiar methods; that some is inspired by the cultural and ethical sciences; and that a few psychologists seem to be striving for the unification of these approaches. Until recently psychoanalysis was the only system that held personality as a central concept, but it is evident that rival theories are in the making. Even though the formulation of really adequate and consistent theories of personality is bound to lag behind special research, this research is now so flourishing that the outlook for a future systematic psychology of personality is bright indeed.

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SPECIAL REVIEWS

KIMBALL YOUNG. *Social Psychology*. New York: Knopf, 1930. Pp. xiv+674.

The content of text-books in social psychology is not standardized, and their systematic positions naturally agree no better than do systematic positions in general psychology. This condition of immaturity in the science has decided Professor Young to design his new text in a fashion that is "not highly theoretical," but descriptive and panoramic. What there is of systematic formulation in the book is eclectic. The author gives his explanations of social behavior at times in terms of prepotent reflexes, conditioning, attitudes, values, and images, and at times in terms of compensation, projection, identification, but most frequently and most emphatically of all, in terms of cultural patterns.

Since the work does not pretend to advance the basic theory of motivation and learning (the process of socialization) upon which social psychology rests, it would be unfair to criticize its lack of thoroughness or cohesion in respect to these explanatory concepts; for example, to point to its glancing attack on suggestion and imitation, or to its facile use of the conditioned reflex. The work seems rather to insist upon, and to illustrate, the rôle of the social environment in framing the mental life of the individual. Some of the most successful chapters deal with race prejudice, neighborhoods, occupational groups, and newspapers. Throughout there is the theme of the potent legend or myth, a favorite theme in social philosophy since the war. With a keen eye for illustrative material, Professor Young has made his argument both interesting and persuasive. His conception of social psychology is a broad one, but his material, as was the case in his *Source Book*, is so well chosen and so ably presented, that even when expanded, the field of social psychology seems in his hands to take on enhanced respectability and authenticity.

I have only one quarrel with the book, and that concerns its indiscriminating use of the term "personality." Personality, like Mesopotamia, is a blessed word; it induces in both the writer and in the reader a sweet sense of stability, security, and modernity. But as the preceding article in this number of the BULLETIN has shown, personality is a not altogether unambiguous concept. In fairness to the psychologists who are struggling to establish its scientific standing it should not be employed merely to round out a sentence gracefully or to stifle a yawn.

Approximately half of the 674 pages in the book are ordered under captions containing the word "personality." The entire discussion of language, institutions, and prejudice, for example, appears under such headings as "Personality and Secondary Groups" or "Personality and Subjective Patterns." A chapter on "Crowd Behavior and Personality" contains nothing that concerns personality proper, but rather an account of certain modifications of the behavior of *all* men in crowds and of the status of *persons* in crowds. Now, personality is not a synonym for individual or for person. An individual is an agglomeration of protoplasm, and a person is primarily a John Doe, who has legal and social standing. Personality, on the other hand, for the psychologist is freighted with uniqueness. Personalities should not be regarded as pawns on the sociological chess-board, restricted by certain ancient rules (cultural patterns) and colored black or white depending upon the interests and prejudices of their particular in-group. In reality a personality, though usually living in a social environment, reflects so many influences, non-social as well as social, that a cultural science cannot deal with it adequately. Personality is a *psychological* organization under cultural, physical, bacteriological, and hereditary influences.

The book opens with the statement that "Social psychology deals with the study of personality as it develops in relation to social environment." On page 201 personality is defined as "the sum total of images, ideas, attitudes, and habits of the individual organized in terms of social participation. Personality, therefore, is an outgrowth of the conditioning of the individual to the personal-social and cultural environment." It is not at all clear that the components of personality must be organized "in terms of social participation" or that personality is an outgrowth merely of the social environment. There are countless interactions between the individual and infra-social nature which both form and express personality. If it cannot fairly be said that personality is exclusively a social phenomenon, neither can it be held that social psychology is identical with the study of personality in its social environment. Many of the problems of social psychology concern *universal* alterations of behavior, motivation common to *all* men, or individual differences in respect to some mental process abstracted from personality. Language is certainly not basically a problem of personality.

Sociologists and biologists in recent years have written much on the distinction between individual and person, and some of this writing Professor Young faithfully records (237f.). A further distinction should be drawn, and observed, between these two con-

cepts and personality. Excepting for the rare occasions on which the peculiar formation of concrete individuality (personality) is the object of their study, sociologists might better employ the concept person or the old favorite, socius. It is the social man or the man with status that interests them, and not the unique individual; it is the typical mentality of neighborhoods, delinquents, voters, with which they work, and not personality.

If these considerations are correct, it is fair to estimate that Professor Young employs the concept of personality in its psychological sense through perhaps one-third of his book; in another third "person" would better have rendered his meaning; and for the remainder, for example in connection with language or myth, neither concept is needed.

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K. D. HAR. *Social Laws*. Chapel Hill: University of North Carolina Press, 1930. Pp. 252.

To anyone interested in the evolution, the rise and fall, the interdependence, or the validity of the generalizations which have been made to account for social processes, Mr. Har's book will be valuable. The book is "an attempt to discover whether there are any valid scientific laws in social phenomena to justify the claim of the various social sciences to the title of positive sciences" (3). After a classification and discussion of some 168 social "laws" and propositions, we are better able to understand the preliminary statement that "if there are any universally valid laws in social phenomena, all the social laws which circulate in one guise or another should be unified on some one fundamental basis. Such a fundamental basis is, to my mind, social psychology" (6). The book has, therefore, a distinctly heuristic appeal to social psychologists.

In order to have some systematic terminology and to avoid the frequent pitfalls of social scientists, chapter II on *The Ideals for Scientific Social Laws* is largely definitive. A scientific social law is conceived as "a description of an invariant pattern of social phenomena, if there be any such invariant patterns, explicable by means of a generally accepted theory of social causation which in turn must be explicable by means of a plausible hypothesis concerning human nature and social relations, thus making the conceptual unification of social phenomena complete" (20). Although he resolves ultimate causal analysis into psychological laws, the author recognizes the need of a pluralistic and coöperative approach of the various social sciences. Psychology may provide political scientists, economists, and anthropologists with a more sound theoretical background

and these may in turn aid psychology by studies of environment and institutional development; even ethics and religion may be contributory.

In chapter III all those laws classed as *apriorisms* and *presuppositions* are discussed. Although such formulations are methodologically necessary and valuable and may lead to a discovery of real social laws, there is the danger that such "laws" will be considered causal. This has been the error of Spencer, Cheyney, Ward, Giddings and others. The same criticism applies to those who have formulated teleological laws (chapter IV) and believed them to be natural "in the sense of being consonant with the imminent principles in the universe" (92). Examples here are the laws of social aim, survival and progress as stated by Blackmar and Gillin; Giddings' "choice of ideals"; Cheyney's "democracy," and "free consent."

Chapter V deals with *statistical laws*; more particularly those of Spencer, Dumont, Malthus, Engels, Streightoff, and Galton are discussed. In all statistical procedure, Mr. Har finds inherent difficulties when the method is applied to social phenomena. Since correlation is not causal relation, since averages may represent an imaginary figure, and since probability is not certainty, this method—although one of the most useful if we are aware of its limitations—is inadequate.

A detailed and competent survey of *near-causal laws* is contained in chapters VI and VII. In the former chapter those laws "based on inferences from the order of historical sequences and those based on inferences from biological and mechanistic analogies" (115) are considered. Here we find Comte's law of the three stages, his laws based on historical sequences and his law of diffusion as well as Spencer's conception of the social organism and Ward's biological metaphors. The author criticizes these laws on the grounds that similarities may be external but not necessarily functional or causal, that many such analogies are contradicted by inductive anthropological studies, and that the reasoning employed is often vague and epistemologically circular.

The criticism of near-causal laws continues in chapter VII with those laws "based on psychological analysis with sympathy as their central principle" (131). Examples here are Giddings' "nosciousness of kind" and all those laws derived from that conception, Ross' "laws of exploitation" and his "laws of social control." Next he takes up those laws "based on psychological analyses with imitation as their central principle" (168) and discusses Tarde, Baldwin,

Cooley, McDougall, Faris, Giddings, and Simons. Analyzing these "laws" Har shows that they seldom go beyond the boundaries of superficial observation, that their explanations are often tautological or include incomprehensible phrases, and that many of the statements are circular. Vividness is often gained at the expense of accuracy.

In the next chapter on *Dialectical Laws as Syntheses*, the author gives an excellent account of Hegel's idealistic version and of Marx's materialistic version of dialectical law and follows them with the dialectical social laws of Ross, Hocking, Giddings, and Cheyney. The reader of *Psychologies of 1930* would find the criticism here applicable to Professor Kornilov's dialectic materialism. In general, Mr. Har considers dialectical law to be a view of life and not a positive law. In it there is a purely logical construction unsupported by facts. The value of such laws would seem to be their mystic spaciousness "as a preliminary step in the search for social laws" (211).

As a means of preserving self-respect and avoiding illusions, Mr. Har proposes in chapter IX that we use the term "social art" as a more accurate designation for "social science." A social art might then be defined as "skill or wisdom which is useful to the appreciation of human values, to the improvement of human relations and the accompanying conditions of living" (212). Among such social arts he lists that of self-annihilation, transvaluation, domination, explosion, separation, and interpenetration. The last would seem to be the most successful as a process of "integrating several conflicting interests, and fulfilling them in their organic unity" (225). As a working basis for these social arts we have all those "social laws" gathered and classified in the previous chapters.

Thus we are shown the limitations of the so-called social laws and come to the conclusion that "the name sociology at present is a mere symbol standing for such an Herculean achievement of the future. As a science, it still remains in the stage of definitive concepts and artistic discussion, without either an adequate technique for study or a sufficient accumulation of data" (51 f.). Since "the ultimate problem to which the study of all social problems can be reduced is the study of man himself" (244), the author believes that causal psychology is the "best available avenue of approach for those who aspire to make sociology an exact positive science" (244). But Mr. Har is not blind to the homeopathic doses which social psychology has injected into the sociological system. Its success in the future will "be measured by its ability to cooperate with the other special social sciences" (244).

HADLEY CANTRIL.

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NOTES AND NEWS

THE death is announced of Dr. Hendrik Zwaardemaker, professor emeritus of physiology at Utrecht. Professor Zwaardemaker died on September 19 at the age of seventy-three. He was best known to psychologists for his work on the sense of smell.

DR. KAI JENSEN of the Department of Psychology at Ohio State University has been appointed research associate for the investigations of the newborn infant.

THE Massachusetts Society for Mental Hygiene announces that through the generosity of the Godfrey M. Hyams Trust it will begin the publication early in January of a quarterly to be called "Understanding the Child." This new magazine will be distributed free to every teacher in the public schools of Massachusetts. Its purpose is to bring to them a knowledge of the principles and practices of mental hygiene. The editorial direction of this new magazine has been placed in an Editorial Board consisting of J. Mace Andress, Editor, and Dr. E. Stanley Abbot and Dr. Henry B. Elkind, Associate Editors. There will be a Consulting Editorial Board of fifteen members, composed of experts in the fields of mental hygiene and education, such as William H. Burnham, C. Macfie Campbell, Augusta F. Bronner, Payson Smith, Lawrence A. Averill and others.

THE Seventh International Conference of Industrial Psychology (Technopsychology, *Psychotechnique*), will be held in 1931 in Moscow, under the presidency of Dr. I. Spielrein of the Institute for the Protection of Labor. While the date has not been definitely determined, it will probably begin on September 15. Industrial psychologists who wish further information regarding this international conference may correspond with American members of the Council, W. V. Bingham, 29 West 39th Street, New York, and M. S. Viteles, University of Pennsylvania, Philadelphia.

By recent action of the National Institute of Psychology Professor R. S. Woodworth of Columbia University was transferred to Honorary Membership. Professor Edward C. Tolman of the University of California was elected to Active Membership.

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